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ON PLATO'S SHIP OF FOOLS.

νόησον γὰρ τοιουτον γενόμενον εἴτε πολλῶν νεῶν πέρι εἴτε μᾶς ναύληρον μεγέθει μὲν καὶ ρόμη ὑπὲρ τοὺς ἐν τῇ νηὶ πάντας, ὑπόκωφον δὲ καὶ ὄρῶντα ὡσαῖτως βραχύ τι καὶ γηγώσκοντα περὶ ναυτικῶν ἔτερα τοιαῦτα, τοὺς δὲ ναύτας στασιάζοντας πρὸς ἀλλήλους περὶ τῆς κυβερνήσεως, ἔκαστον οἰόμενον δεῦν κυβερνᾶν, μῆτε μαθόντα πῶποτε τὴν τέχνην μῆτε ἔχοντα ἀποδέξαι διδάσκαλον ἔαντον μηδὲ χρόνον ἐν ὧ ἐμάνθανε, πρὸς δὲ τούτους φάσκοντας μηδὲ διδακτὸν εἴναι, . . . αὐτὸς δὲ αὐτῷ ἀεὶ τῷ ναυκλήρῳ περικεχύσθαι, δεομένους καὶ πάντα ποιοῦντας, ὅπως ἀν σφίσι τὸ πηδάλιον ἐπιτρέψῃ . . . πρὸς δὲ τούτους ἐπαινοῦντας . . . ὃς ἀν ἔνδλαμβάνειν δεινὸς ἔη, ὅπως ἄρξονται η πείθοντες η βιαζόμενοι τὸν ναύληρον. . . τοῦ δὲ ἀληθινοῦ κυβερνήτου περὶ μηδὲ ἐτάσσοντες ὅτι ἀνάγκη αὐτῷ τῇ ἐπιμέλειαν ποιεῖσθαι ἐναυτοῦ καὶ ὥρων . . . ὅπως δὲ κυβερνήσει ἐάν τέ τινες βούλωνται ἔαν τε μή, μῆτε τέχνην τούτου μῆτε μελέτην οἰόμενοι δινατάντο εἴναι λαβεῖν ἄμα καὶ τὴν κυβερνητικήν.—Plato's *Republic*, 488.

The familiar allegory of The Ship of State in Plato's Republic (Book vi, 488), has been so persistently perverted in British publications, that it seems to deserve a brief restatement. Mr. Grote in his 'Plato' (iv. 53), paraphrases the parable as follows: 'Conceive a ship on her voyage, under the management of a steersman distinguished for force of body as well as for skill in his craft, but not clever in dealing with, or acting upon other men...They never reflect that the genuine steersman...cannot at the same time possess skill and practice in keeping his hold of the rudder whether the crew are pleased with him or not.' Mr. Grote's interpretation shows that he understood the

'steersman distinguished for force of body' etc. to be the true philosopher, and this doubtless it was which led him to misunderstand and insert the 'skill in his craft.'

Mr. Jowett's translation, essentially unchanged in the different editions, runs as follows: 'Imagine then a fleet or a ship in which there is a captain who is taller and stronger than any of the crew, but he is a little deaf and has a similar infirmity in sight, and his knowledge of navigation is not much better. The sailors are quarrelling with one another about the steering...they mutiny' etc. ' . . but that the true pilot must pay attention to the year and seasons . . . and that he must and will be the steerer whether other people like or not . . the possibility of this union of authority with the steerer's art has never seriously entered into their thoughts...Now in vessels which are in a state of mutiny, and by sailors who are mutineers, how will the true pilot be regarded ?'

In the margin of this translation and in that of the text of the Jowett and Campbell edition, we read: 'the noble captain whose senses are rather dull (the people in their better mind) ; the mutinous crew' etc.

Mr. Purves in his 'Selections from Plato' (both editions) paraphrases thus: 'I will compare the philosopher to a pilot, who is a very good pilot, but rather deaf and parcel-blind, and so is unable to control his mutinous crew,' etc. And, again, 'but somewhat deaf and parcel-blind to match, and no great adept in navigation.' And, once more: 'The meaning of the whole passage is: "These mutineers are in a double ignorance. They have no conception of the art of the

true pilot; and if they had, they deem such art incompatible with any art or practice of preserving order whether the ship's company consent or no." This explanation of Mr. Purves is most perplexing. How can a pilot be a 'very good pilot,' when he is 'rather deaf, parcel-blind to match, and no great adept in navigation?' And how can a philosopher be likened to such a pilot? If Adimantus understood Socrates to mean this, we cannot wonder that he was surprised at his assertion that philosophers should rule the State.

But what does Plato mean? Simply that the people of Athens are like a ship-owner who has little knowledge of navigation; they are more powerful than any one else in the ship of State, if they care to exercise their authority: they have the right to appoint the ruler of the State, as the ship-owner had the right to appoint the captain for his vessel; but they do not understand very well what is told them, have little foresight, and know little of the principles of government. The crew are the politicians, each of whom desires to command the ship of State, though he has never learned the art of ruling, says even that this cannot be taught, that there is no such art. The politicians regard as an able man and a true 'statesman' one who is able to 'pack the primaries' and 'lay wires' in such a way that they may secure control of the government; they call an unpractical visionary him who has no skill in securing nominations and elections, though he may be fit to rule by nature, and is acquainted with all the principles of government. On the other hand, the philosopher, the true seaman, does not believe there is any true art in the devices to attain power whether the other party desires it or not; and that in any case the man who is to be qualified to command the ship of State needs all his time for higher duties and has none to devote to the mere attainment of power. As Socrates goes on to say only a few lines farther on, nature forbids the true seaman to beg sailors to receive his commands; he will not seek office; if a man is ill he must seek the physician, and if a man needs to be ruled he must seek a ruler, *οὐ τὸν ἄρχοντα δεῖται τὸν ἀρχομένον ἄρχεσθαι, οὐ ἀν τὴν ἀληθείαν ὁ σφέλος γένεται.* On these grounds the philosopher is indeed useless to the State, but only because the State does not commit itself to him.

What has caused the misinterpretation of the passage in which we find nothing about a 'noble captain' and 'mutinous

crew,' and nothing about 'preserving order whether the ship's company consent or no,' and where the ship-owner represents the people not 'in their better mind,' but in their ordinary mind? For one of the sailors to persuade by any means the owner of the vessel to appoint him its captain, is not mutiny—even if wine or liquors should be used to this end.

The chief trouble of the exegetes of this passage seems to have arisen from the word *ναύληρος* *ship-owner*, with some collateral doubt about *κυβερνήτης*, which at times means *helmsman* or *pilot*, though it is usually applied to the *commander* of a boat (compare the use of *governor* in Latin). In early times the 'skipper' owned his own boat; the owner was also the captain of the ship, and *ναύληρος* in the fifth century B.C. might be used for a sea-captain. In the next century, as commerce became more important, more frequently a man who knew nothing about nautical matters invested his money in a ship, and appointed a seaman to manage it. (See Plutarch *Morals* 807 B, *ναύτας μὲν ἐκλέγεται κυβερνήτης, καὶ κυβερνήτην ναύληρος.*) Not infrequently he accompanied the ship, in order to watch his property and maintain his rights. In a notable passage in Demosthenes's oration 'On the Crown' (§ 194), the orator compares himself to a *ναύληρος* who had provided his boat with all that he supposed necessary for safety, but was blamed for the wreck; *ἀλλ' οὐτ' ἐκνέρων τὴν ναῦν, φῆσεν ἄν, ὥσπερ οἰδ' ἐστρατήγουν ἔγω.* That the *ναύληρος* was with his vessel at the time of the disaster is implied in his excuse—not that he was absent but that he was not *in command*, *οὐτ' ἐκνέρων*, which again illustrates the meaning of *κυβερνᾶν* in our passage. Another excellent illustration of the use of the two words is found in the Acts of the Apostles, xxvii. 11, where 'the centurion believed the master and the owner of the ship (*τῷ κυβερνήτῃ καὶ τῷ ναυκλήρῳ*) more than those things which were spoken by Paul.'

For the latter part of the passage, Mr. Jowett offers alternative translations, and the commentary of Jowett and Campbell offers alternative constructions, all of which I am obliged to consider faulty. The alternative translation given in a footnote is: 'But only understanding (*ἰσταίνεται*) that he (the mutinous pilot) must rule in spite of other people, never considering that there is an art of command which may be practised in combination with the pilot's art.' This really seems no improvement on

'that he must and will be the steerer, whether other people like or not,' etc. The subject of *ὅπως δὲ κυβερνήσει* must be, not a 'mutinous pilot,' but the true pilot, the philosopher. *ὅπως κυβερνήσει* is not 'a resumption' (Jowett-Campbell) of *ὅπως ἀρξόντων*, but is parallel with it, and both verbs alike have the inceptive force, *attain power, obtain control*. The thoughts form a balance, not a repetition. Mr. Grote's translation, 'keeping his hold of the rudder,' and Mr. Jowett's, 'must and will be the steerer,' both hide and blunt the very point which Socrates is urging. Of course the Greek future can denote attainment as well as continuance of an action; the connexion of thought must decide.

The Jowett-Campbell commentary is quite right in saying that *πειθούτες η̄ βιαζόμενοι* is parallel with *ἔαν τέ τυves βούλωται ἔαν τέ μή*, but it is wrong in its inference from this remark; the latter of the expressions refers to the true pilot, while the former refers to the disorderly crew. The politicians are ready to acquire office by all means, by persuasion or by force, while the true Platonic statesman will not spend his time in planning to secure an election against all opposition. The politician thinks there is no art of ruling the State, and scorns the man who differs with him; the philosopher, the statesman, on the other hand, believes that adroitness in gaining office is no true art, and he despises such skill.

Up to this point, the writer is confident that the reader agrees with him. The mere statement of the alternatives would seem to suffice. No scholar after consideration can doubt that *ναύκληρος* here is *ship-owner*, and does not, like 'captain,' imply the direction of the ship in detail; nor that *κυβερνήτης* is *sea-captain*, and not a mere helmsman, who simply obeys the captain's orders as to the course, and has no need to study the heavens and the seasons; nor that it is the philosopher and not the ordinary politician who disbelieves the possibility of uniting the skill of the true ruler (who, according to Plato, never considers what is for his own advantage, but only that which is good for the ruled) with adroitness in securing place and power. But the word for *believing* in this last sentence (488 E) is of uncertain grammatical case in the tradition: the good MSS. have *οἰόμενοι*, others of less authority *οἰομένοντος*, and another *οἰομένον* in the margin. If we read *οἴόμενοι* with the authoritative MSS., Schneider, C. F. Hermann, and Burnet, we must suppose a change in the construction, from a dependent to a more

independent statement, and from the particular man to the class of which he is one. The accusative *οἰομένος* clearly agrees with the disorderly crew, like *ψέοντας* above, and so is impossible. The genitive *οἰομένον* would agree with *τοῦ ἀληθινοῦ κυβερνήτου*, six lines above, which is demanded by the sense, as we have seen; it is so slightly attested, however, that we are forced to believe it the conjecture of a scribe. The dative *οἰομένῳ* was conjectured by Mr. Henry Sidgwick (*Journal of Philology*, v. 275), who urged correctly that 'it must be the true statesman who sees the impossibility of acquiring along with his proper art the quite different art of getting and keeping power.' But the agreement with *αὐτῷ* is not easy, and this suggestion is declared impossible by Mr. Richards in his 'Critical Notes on the *Republic*' (*Classical Review*, viii. 23). Mr. Richards, by the way, presents a conjecture in which he had been anticipated by Schmelzer, of *ἀδύνατον* or *οὐ δύνατόν* for *δύνατόν*, making the disorderly crew believe it possible to attain skill both in securing power and in ruling. This, however, not only destroys the balance of the sentence but is inconsistent with their belief previously asserted that there is no learning of *κυβερνητική*. Either *οἰομένοι* or *οἰομένον* satisfies the demands of the thought, but the nominative *οἴόμενος*, explained as under the influence of the relative clause, *ὅπως κυβερνήσει*, would be more Platonic, and would be subject to assimilation by the scribes to the plural of the other participles. For such attraction it may be sufficient to refer to Riddle's 'Digest of Platonic Idioms,' § 192; in the *Classical Review* for 1901, page 28, I called attention to some examples of attraction where the attracting power seems quite as remote. The meaning of Plato is distinct; the form of one word which he used, is uncertain.

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P.S.—This passage of the *Republic* has been used as an illustration for Aristotle's *Politics* 1324b 22, but a careful examination shows the later passage to be a distinct reminiscence of the former. Cf. *καίτοι δέξειν ἄν ἄγαν ἄποτον ἵως ἔιναι τοῖς βουλομένοις ἐπισκοπέν*, *εἰ τοῦτ' ἐστιν ἔργον τοῦ πολιτικοῦ, τὸ δύνασθαι θεωρεῖν ὅπος ἄρχῃ καὶ δεσπόζῃ τῶν πλησίον καὶ βουλομένων καὶ μὴ βουλομένων. πῶς γὰρ ἀν ἐίη τοῦτο πολιτικὸν ἢ νομοθετικόν, ὃ γε μηδὲ νομιμόν ἐστιν;...οὔτε*
c c 2

γὰρ τοῦ ἱατροῦ οὐτε τοῦ κυβερνήτον ἔργον ἔστι τὸ ἡ πέισαι ἡ τὸ βιάσασθαι τοῦ μὲν τοῦ θεραπευομένους τοῦ δὲ τοὺς πλωτῆρας. This seems strongly to support the contention that the Platonic *οἰομένους* refers to the true

pilot, while the Platonic passage would indicate that the subject of the Aristotelian *ἀρχή* was not the indefinite *τις* (so Newman) but δ *πολιτικός*.

THE 'ANCIENT VULGATE' OF PLATO AND VIND. F.

PROFESSOR BURNET's article on 'A Neglected MS. of Plato,' in *C.R.* xvi., 98 ff. and Mr. Adam's review of Prof. Burnet's text of the *Republic* in *C.R.* xvi., 215 ff. have initiated the discussion of a question whose solution is of the highest importance to the textual critic of Plato. Prof. Burnet, in his introduction, repeats with approval the statement of Schneider with regard to Vind. F. 'Veterem vulgatam repraesentat, et fere cum Stobaeo, Eusebio etc. consentit.' To this he adds, 'Mirum in modum consentire solet hic liber cum Iamblichi Galeni Stobaei Eusebii aliorum testimonii, quae res vel ex mea adnotatione satis apparebit,' and thereupon hails with joy 'diu quae situm antiquioris recensionis testem.' In *C.R.* xvi. 99 Prof. Burnet lays down the proposition that 'the original of Vind. F. was not only older than our oldest MSS.; it was quite independent of the common archetype of ADM, and followed the recension used by Galen, Iamblichus, Stobaeus, Clement, Eusebius and other writers of the first five centuries after Christ.' He then adduces the agreement of W¹ with the Armenian version on the one hand and ancient quotations on the other as a proof of the theory that 'there was an "ancient vulgate" of Plato's text, while our MSS. represent a recension made possibly about the ninth century A.D.'

Mr. Adam, in his review of Prof. Burnet's text, concedes 'for the sake of argument' that F represents the 'ancient vulgate,' although he afterwards speaks hesitatingly as to the existence of such a 'vulgate,' and confines himself to arguing that its authority is at least no greater than that of the old MSS.

It appears to me that the previous question as to the existence of an 'antiquior recensio' of the Platonic text, used by the writers of the first five centuries A.D., deserves a closer examination than it has hitherto

¹ W does not, of course, contain the *Republic*. Prof. Burnet had drawn attention to the fact, and deduced from it the theory of the 'Vulgate,' in the preface to his first volume.

received. It would no doubt be necessary, in order to a final solution of the problem, to undertake a thorough examination of the text, not merely of the *Republic*, but also of other dialogues; Prof. Burnet, however, claims that his own *apparatus criticus* furnishes a sufficient proof of his thesis, and it would certainly seem that the facts therein set forth do enable us to form a judgment on the question at issue, though not, as I believe, that upheld by Prof. Burnet.

First let me say that it seems necessary to set out with a strictly defined conception of what is meant by the term 'vulgate.' As applied to printed books the word denotes a 'commercial' text reproduced in successive editions with no pretence at critical revision. The distinguishing mark of such a text is naturally its uniformity. In the ancient world the same phenomenon may be observed in the transmission of classical texts; the Homeric 'vulgate' affords a conspicuous example of a 'commercial' text such as those to which I have referred: but owing to the want of a purely mechanical process of reproduction the limits of variation are much greater than in modern books. At the same time, as has been shown by Mr. Allen and others in the case of Homer, such a 'commercial' text became so far fixed that the collation of a few MSS. of no special excellence would be sufficient to produce a text satisfactory on the whole.

We have now to ask firstly, whether such a text of the Platonic dialogues obtained currency in ancient times, and secondly, whether (if so) it is represented in the case of the *Republic* by Vind. F.

The witnesses to whom Prof. Burnet appeals in giving an affirmative answer to the first question are certain writers of the Christian era. The earliest of those whom he names is Galen; of the others two are Neo-Platonists, two Ecclesiastical writers and the last the compiler Stobaeus. Quotations from the *Republic* are also found in writers other than those enumerated by Prof. Burnet, e.g., Plutarch, Justin, Athen-

aeus, Theon and Proclus. It must be admitted that if proof could be adduced that these writers, belonging to different centuries and schools of literature, were in possession of a substantially uniform text of Plato, there would be considerable justification for speaking of an 'ancient vulgate' of Plato. But is this the case?

There is no passage in the *Republic* for which so much external evidence is available as the Vision of Er. The *testimonia* are drawn from Plutarch, Theon, the Fathers of the Church, the Neo-Platonists and Stobaeus, so that here if anywhere we shall find a test case by which to try Prof. Burnet's theory. Now his *apparatus criticus* reveals as plainly as could be desired the fact that these authors do not quote from a 'vulgate' text. The Ardiaeus of Proclus, for example, is Aridaeus in the text used by the Christian writers. A number of readings are quoted by Prof. Burnet as supported by one ancient authority only; and the extent of the variation between the texts current in later antiquity may be gauged by such instances as the following:—

614^a 8 οὐν Proclus Stobaeus; of Eusebius Theodoretus.

615^a 3 χιλιέτη Proclus: χιλιέτων Stobaeus.

615^a 1 οὐδὲν ἡξει Justinus Stobaeus: ἀν om. Proclus.

615^a 6 δαλαβόντες Proclus: ἴδια λαβόντες Stobaeus; ἴδια παραλαβόντες Clemens Eusebius.

616^a 3 κνάμπτοντες Proclus Clemens Eusebius: γνάμπτοντες Justinus.

616^b 6 προσφερῆ Proclus Stobaeus: ἐμφερῆ Theo.

No passage, as has been said already, furnishes so ready a means of testing the accuracy of the theory which postulates an 'ancient vulgate': but there are other important variations in the text of ancient quotations which point in the direction indicated above. Thus the reading ἀποδράσ, adopted by Prof. Burnet in *Rep.* 329^c 4, is due to Theon, whereas ἀποφυγών is found in Clement and Stobaeus. Which is that of the 'vulgate'? There are no doubt passages in which various ancient writers appear to have had the same or similar recensions of the text before them. Thus in ii. 361 the Christian writers generally agree in readings which differ from those of our MSS.; yet even here Eusebius and Theodoret disagree in two places (361^b 5 and 361^c 5). In iv. 422 Eusebius and Stobaeus agree in three readings but disagree in three others. In vii. 531 Theon and Eusebius disagree in

every important case. Certainly there is nothing in such facts as these—and they are typical—to countenance the theory that the text used by the writers of the first five centuries A.D. partook of the nature of a 'vulgate'.

We now pass to the consideration of Vind. F and its readings. The question, indeed, which we proposed at the outset no longer demands an answer, since the existence of an 'ancient vulgate' appears more than doubtful. At the same time it is not to be denied that in a number of instances the reading of F coincides with that of one or more ancient writers and differs from that of ADM. When, however, we examine these instances we find that the statements quoted from Prof. Burnet at the beginning of this article cannot be maintained without modification. It will be convenient to treat the cases of different ancient writers separately.

I. Galen's testimony is available in the following sections of the *Republic*.

(a) II. 368. Here he has the following readings:—δοκῶ 368^a 1 (adopted by Prof. Burnet): ἐνὸς ἀνδρὸς 368^a 2: παλιν τῆς 368^a 3: μείζων 368^a 5 (so too Proclus): ἀν εἴη 368^a 7. *In no case does F agree with Galen.*

(b) III. 407. Galen has:—τί γε for γε τί 407^b 4 in which F agrees, and διατάσσεις 407^c 2, to which reading F has been corrected.

(c) III. 410. Galen has:—γε μὴν (adopted by Prof. Burnet) 410^b 5: κάκειν ἐπεγείρων 410^b 6: μεταχειρίζονται 410^b 8. *In no case does F agree.*

(d) IV. 435/436. Galen has:—γ' omitted 435^c 9: αἰσ δὴ 435^a 1: ἀλλη (an important variant, adopted by Prof. Burnet) 435^a 3: τε 436^a 9: διομολογησώμεθα 436^a 8: στρόμβοι 436^a 5: ἀποδεχομέθε (adopted by Prof. Burnet) 436^a 8: καὶ (om. MSS., adopted by Prof. Burnet) 436^a 4: ἔστι omitted 436^a 6. *In no case does F agree.*

(e) IV. 439/441. Galen has:—θηρίον 439^b 4 (so too Stobaeus): τῷ γε αὐτῷ τῷ αὐτὸν τῷ 439^b 5: πράττειν 439^b 6: εἴη ἀν (for ἀν εἴη) 439^b 4: τοῦτο 439^b 6 (so too Stobaeus): αὐτὸν omitted 439^b 9 (so too Stobaeus); μὲν 439^b 10 (so too Stobaeus): <χαλεπαίνειν καὶ> πολεμεῖν 440^b 5: πολλάκις 440^b 8: ζεῖ (adopted by Prof. Burnet) 440^b 7: νικᾶν...λήγειν 440^b 1: ἔκει omitted 441^b 4. *In only two readings (εἴη ἀν for ἀν εἴη, and μὲν [om. MSS.]) does F agree with Galen, while M agrees with Galen and Stobaeus in τοῦτο.*

and D with the same authorities in omitting *αῖ*. Moreover the readings of Galen and Stobaeus, though agreeing in the cases mentioned above, are widely divergent in several others, so that in this passage at least it would be hard to maintain either that an 'ancient vulgate' was in existence, or that it was 'represented' by any one of our MSS.

(f) V. 451—456 *passim*. Galen has:—
ταῦς μὲν (om. MSS.) 451^a 1: *τε* omitted 452^a 2: *τε* (om. MSS.) 452^a 5: *λέγομεν* 453^b 2: *κατοκίσσως* 453^b 4: *δ'* omitted 453^b 9: *ννὶ* 453^b 3; *τείνον τὰ* (adopted by Prof. Burnet) 454^a 1: *ἔχοντα* 454^a 2: *γε* (om. MSS., except F) 454^a 5: *γ'* (om. MSS., except F) 454^a 5: *δλύψ* 455^a 5: *τοῦ* 455^a 5: *πάμπολν* (so to *τοῦ* Eusebius) 455^a 1: *τι* 455^a 4: *δὲ ἀρ' ή* 456^a 1: *καὶ* (for *οὐδὲ*) 456^a 1: *ή μὲν* (om. MSS. and Eusebius) 456^a 11: *δ' ὅτι* 456^a 11: *αι* om. 456^b 1: *φυλάττειν* 456^b 2. Here we have some minor agreements with F; twice the particle *γε* is found in both authorities, but not in the old MSS.: the two remaining cases are the common errors *λέγομεν* for *λέγομεν* and *κατοκίσσως* for *κατοκίσσως*, which are in all probability the result of coincidence. For none of the really important variants in the latter part of the passage is common to Galen and F: while the texts of Galen and Eusebius differ materially.

(g) IX. 581. Galen has *ὅτι* 581^a 6: *τοῦ* omitted 581^a 6. In neither case does F agree.

II. Iamblichus furnishes a considerable amount of external testimony to the text of book VII. We extract the following readings from Prof. Burnet's *apparatus criticus*.

ταρὰ πᾶν 514^a 4: *ἥν εἴναι* 514^b 4: *φόδομη-*
μένον 514^b 4: *δεικνύονταν* 514^b 6: *οὐ ταῦτα*
515^b 4: ὄντα 515^b 5 (and probably also Proclus): *δνομάζειν* 515^b 5 (where F and Proclus have *νομίζειν*): *ή* 515^b 5: *αὐτὸ* 515^a 2: *τε* (for *τότε*) 515^a 6: *πάντως δήπον* (for *πολύ γ' ἔφη*) 515^a 8: *ννὶ* omitted 516^a 3: *τεληρῶν* 516^a 1: *τε* (om. MSS.) 516^a 6: *ἥσαν* *αὐτοῖς* 516^a 8: *περὶ* 516^a 8: *εἴωθε* 516^a 1: *καὶ* omitted 516^a 7: *ἀμβλυωπεῖ* 516^a 9: *ταρέχοι* 517^a 2: *ἀνέναι* 517^a 4: *ώς ἀληθῶς τοῖς* (for *τοῖς ἔμπροσθεῖ*) 517^a 1: *αὐτὴ* (for *αὐτὴ* or *αὐτῆ*) 517^a 4: *παρεχούμενη* 517^a 4: *νοῆσαι* 518^b 6: *οτὸν* 518^b 7: *σφέας* omitted 518^b 1: *ἐκάστῳ* 518^b 5: *τὸ* omitted 518^b 5: *δεῖ μη-*
χανῆσασθαι 518^b 7: *μὲν* transposed 519^a 2: *τὰ* (om. MSS.) 519^b 2: *φιλοράθειαν* 521^a 6: *ἀληθινὴν* 521^a 8: *επ'* (adopted by Prof. Burnet) 523^a 1: *οὐδὲ* *ἔγερτικὸν* omitted 523^a 1: *αὐτῶν τὰ* 523^a 4: *ή μαλακότητα* omitted

523^a 6: *δηλώσονταν* 523^a 7: *αὐτὴ* 524^a 7: *μεταχειρίσασθαι* 526^a 7: *εἰσιν* omitted 527^a 5: *ἔτι ἀδναύις* (adopted by Prof. Burnet) 532^a 9: *τὸ* (for *τότε*) 532^a 6: *ἀδοάτῳ* 532^a 1: *καὶ* omitted 536^a 5: *φιλομαθείας* 536^a 5: *πασιν* omitted (so too Stobaeus: but Theon has *πᾶσιν* and F reads *πασι*) 537^a 1: *τε* (found in F Theon Stobaeus, but omitted by ADM) 537^a 2. Out of all these readings only

four are found in F: one is *αὐτὴ* for *αὐτὴ* (*αὐτὴ* D), two are omissions of *καὶ*, one—and the only one which we might perhaps call that of the 'ancient vulgate'—the addition of *τε*. Not one of the really important variants given by Iamblichus recurs in F. Iamblichus' testimony is also available for IX. 589—591. The following readings are found in his text: *ἴαντῷ* 589^a 4 (*ἴαντῷ* MSS., *αὐτὸν* Stobaeus): *εἰς ἐλενθερίαν* (also Stobaeus: omitted by MSS.) 590^a 7: *βούλε-*
ται (also Stobaeus and [possibly] F) 590^a 1: *τῶν* 591^a 1: *τούτῳ* 591^a 1: *τὰ* omitted 591^a 2: *ζῆσαι* (adopted by Prof. Burnet) 591^a 7: *φανεῖται* (adopted by Prof. Burnet) 591^a 3: *εἰς* (om. MSS.) 591^a 8. Only in the doubtful case of *βούλεται* does F agree with Iamblichus: while in the important variant *ζῆσαι*—the true reading—Iamblichus is supported not by F but by M, just as *δεῖ μη-*
χανῆσασθαι is read by A and Iamblichus in 518^a 7.

I have examined the cases of Galen and Iamblichus with special minuteness because the results are such as to demonstrate beyond the possibility of doubt that Prof. Burnet's statement as to F—mirum in modum consentit cum Iamblichi Galeni...testimonio—cannot be upheld. It will not be necessary to deal minutely with the cases of the other ancient writers who cite passages of the *Republic*. The principal of these are Eusebius and Stobaeus: and it may be said at the outset that it would no doubt be possible to frame a not inconsiderable list of passages in which the reading of F agrees with that of one (or perhaps both) of those writers. At the same time the following propositions may also be laid down without fear of contradiction.

(a) That the cases of agreement between F Eus. and F Stob. are much less frequent and less important than those in which they differ: for example the consensus of F Stob. in the reading *τὸ* for *τὰ* 444^a 2 is followed by a series of the widest divergences down to the end of Book IV.

(b) That the text to which Eus. and Stob. bear witness is on the whole very inferior

to that of our MSS. I should not, it is true, describe it as an 'ancient vulgate,' but I should at any rate assert that it represents the 'commercial' texts which circulated amongst the reading public rather than the more scholarly editions in the hands of Galen and Iamblichus.

(c) That in a considerable proportion of the instances in which the reading of F Stob, (or F Eus.) is obviously to be preferred to that of ADM, the palaeographical difference involved is too small to afford any presumption of a distinct recension used by the scribe of F and by the ancient authorities. In other words—it may be conceded that F in several cases preserves the true reading where it has been corrupted in ADM, and further that in a certain proportion of these the right reading is confirmed by ancient quotations. For example, both F and Stobaeus have φαμέν (rightly) in 490^c 2 against φαίμεν (wrongly) in 490^c 2: yet no one would affirm that φαμέν was the reading of the 'ancient vulgate' merely: φαίμεν is an example of an extremely common error. So too F and Eusebius have ἔθη (rightly) in 522^a 7, where ADM by a very frequent corruption have ἔθη—certainly not owing to any 'recension.' It is unnecessary to multiply such examples, which I merely adduce in proof of the fact that the agreements of 'F Stob,' 'F. Eus.' &c., which are to be found in Prof. Burnet's critical apparatus are not all of equal value in demonstrating the relationship of F to an assumed 'vulgate': many of them prove

only (what indeed all scholars will be grateful to Prof. Burnet for bringing into strong relief) that F must rank as a witness independent of ADM.

And this leads me to add a word in conclusion as to the value to be assigned to this MS., whether we regard it as the ancestor of Ang. B and Flor. R or, as Mr. Adam suggests, merely the senior representative of the group. I have already said that Prof. Burnet has rendered a great service to students of Plato by employing it to correct the errors of ADM and thus to a considerable extent rendering an appeal to the MSS. of the Renaissance (esp. Ξq) superfluous. An examination of its readings must, I think, convince the open-minded that it follows a tradition not derivable from ADM. At the same time I cannot think that it is safe to pronounce it unhesitatingly (as Prof. Burnet does in his preface) 'sine nec aliunde ut fit interpolatum.' The well-known reading <ού> κατὰ τὴν χρείαν 330^c 6 (which is not supported by Stobaeus) bears the stamp of an unintelligent conjecture, and is in fact found in the MSS. of which such unfortunate emendations are characteristic. Another instance in which the scribe of F or its ancestor has endeavoured to improve the traditional text is ἀδικεῖν ἀδικεῖ for ἀδικεῖν ἀδικεῖ 360^b 8. We must therefore, I fear, regard its unsupported readings with something (though, as I allow, with relatively little) of the scepticism with which those e.g. of Ξq are rightly treated.

H. STUART JONES.

ARETHAS AND THE CODEX CLARKIANUS.

UNDER this title Professor Burnet, the learned editor of the Oxford Plato, has commented in the June number of the *Classical Review* on a short article which I had contributed to the February number 'On some corrections in the Clarke MS. of Plato.'

From the fact that both this famous Codex and the oldest MS. of the *Praeparatio Evangelica* of Eusebius were written for Arethas, the one in A.D. 895, and the other in A.D. 914, I was naturally led to compare carefully the texts of the principal passage common to the two MSS., *Phaedo* 96 A—C = Eus. P.E. 26 b 8. I thus became convinced that certain corrections in *Clarkianus* (1) were

written by the hand which is generally admitted to be that of Arethas himself, and (2) were either taken from or 'made to correspond with the text of Eusebius as reproduced by the scribe Baanes from some older MS. now lost.'

On the former point Professor Burnet writes that he has 'no desire to dispute this belief which is held by many scholars of repute,' but then proceeds to give reasons for still hesitating to accept it. Those reasons had been considered and, I think, sufficiently answered in my article; but in order to remove all lingering doubt as to the probability of the rich owner of an important MS. writing corrections with his

own hand, it may be well to refer to one or two well-known examples.

‘A colophon found in an extant Vatican MS., and given in fac-simile in Migne’s Eus. (Oyp. iv. 876) presents a lively picture of the common labours of the two friends’ (Pamphilus and Eusebius) ‘at this time. “It was transcribed from the editions of the Hexapla, and was corrected from the Tetrapla of Origen himself, which also had been corrected and furnished with Scholia in his own handwriting, whence I Eusebius added the Scholia. Pamphilus and Eusebius corrected this copy”’ (Lightfoot, *Dictionary of Christian Biography*, i. 310 a).

Another example exactly corresponding to the case of Archbishop Arethas is found in the subscription of the famous MS. of Plato, A Paris. n. 1807, written about the same time as *Clarkianus*: ‘Ορθόθη ἡ βίβλος αὗτη ἵππο Κωνσταντίνου μητροπολίτου Ιερᾶς πόλεως τοῦ καὶ ὀνταμένου.

On the second point mentioned above, namely that the corrections in question were probably derived by Arethas from his MS. of Eusebius, Professor Burnet says: ‘It can be shown that Dr. Gifford’s argument does not tell one way or the other.’

The proof which Professor Burnet offers in support of this assertion is an attempt to show that the readings in question may have come from a ‘more obvious source’. They ‘are not,’ he says, ‘in any way peculiar to Eusebius; they are the readings of the great majority of Platonic MSS.’ . . . ‘In particular the supposed Eusebian readings of the diorthotes (whom I shall call B²) are in every case the readings of the *Marcianus* (App. Class. 4. 1), which Schanz calls T, and also for the most part of Stobaeus.’

Professor Burnet will, I hope, admit that, however common these readings may have become in later MSS., the first place in which they are found is the extract from the *Phaedo* in the MS. of Arethas. Now this extract must have been originally copied by or for Eusebius *circ.* A.D. 315, and therefore from some MS. nearly six centuries older than *Clarkianus*; and the MS. of Arethas in which these Eusebian readings stand in the body of the text is all but contemporary with *Clarkianus*, two hundred years older than *Marcianus* (Cent. xii.) or any other MS. of Plato.

How then can it possibly be maintained that *Marcianus* (T) is a ‘more obvious source’ of these readings than the corrections made by Arethas in the oldest and best MS. of the *Phaedo* two hundred years before?

Professor Burnet states indeed that ‘T is independent of B and B²’ (i.e. of *Clarkianus* and its corrections), ‘as has been clearly proved by Schanz and others.’

Even if this were admitted, it would not help to prove that either T or some unknown ‘older MS. now lost, from which T also was derived,’ is a ‘more obvious source’ of the corrections in *Clarkianus* than the Eusebian text known to have been in the hands of the corrector himself. Professor Burnet, however, gives no reference to the places in which, he says, T is proved to be independent of B and B²: and on consulting the careful description of T in Schanz, ‘Ueber den Plato-codex der Markusbibliothek in Venedig append. class. 4, n. 1, Leipzig. 1877,’ I can find no suggestion that T is independent of B and B². In fact what I do find tends to the opposite conclusion.

On p. 78 from several passages there quoted Schanz draws the following inference: ‘Diese Stellen legen den Schluss nahe, dass t (T) aus A stammt,’ i.e., that T is derived from the famous Paris MS. of Plato, not both A and T from some third unknown source.

At the first glance this might seem to show that T being derived from A is therefore independent of *Clarkianus* and its corrections. But unfortunately for any such inference the *Phaedo* is not one of the dialogues contained in A.

On p. 38 Schanz states that corrections already existed in the source from which T was copied; and on p. 49 he quotes four passages in which T is actually corrected from *Clarkianus*, adding the remark, ‘Of course it is not likely that the activity of Arethas in commenting on Plato was limited to these three Scholia.’

If this is the testimony of Schanz to which Professor Burnet refers, it is very far from supporting his assertion that ‘T is independent of B and B²;’ and so far as his argument is based on this assumption, it seems to have no real foundation.

But Professor Burnet brings forward another argument from passages not quoted by Eusebius, in which similar errors of Calligraphus are corrected by B², i.e. by the hand which has been identified with that of Arethas and by which the *Phaedo* was corrected throughout. See foot-note.¹

‘These corrections,’ Professor Burnet says,

¹ ‘Unum illud addo scribam A’ (B²) ‘dialogis Euthyphroni, Apologiae, Critoni, Phaedoni litteram Δ subjunxisse, quia ut doceo conjecte Waddell διωθεούσενον vel διώθωσα significatur’ (T. W. Allen, *Praef. Cod. Clark. phototyp. ed.*, p. 5).

'are not conjectural; for they are identical as a rule with the readings of T. Surely the inference is that the diorthosis of the *Clarkianus* was made from an older MS. now lost, from which T was also derived.'

It is difficult to estimate the validity of this inference without further information; for, as Professor Burnet again fails to give any references, we have no means of judging for ourselves. Granting, however, that these corrections in *Clarkianus* were made 'from an older MS.', why must it be a MS. 'now lost,' the unknown source of T? Why not rather the contemporary MS. of Eusebius *P.E.* still existing and known to have been in the hands of the corrector, or possibly the MS. of Plato given out to be copied and now lost?

The reason alleged by Professor Burnet is far from conclusive and apparently not quite convincing to himself: 'That it was not the same as the MS. given out to be copied may perhaps be implied by the fairly common $\epsilon\nu\ddot{\alpha}\lambda\omega$, but it is not very probable that $\epsilon\nu\ddot{\alpha}\lambda\omega$ at *Phaedo*, 96c 7 means "in my copy of Eusebius" and something quite different elsewhere.'

I should have thought that both here and elsewhere, wherever $\epsilon\nu\ddot{\alpha}\lambda\omega$ occurs in the margin, it means simply 'in another' MS., and therefore different MSS. in different works. Here the only sources to which, as far as we know, the corrector had access were his copy of Eusebius and the MS. of Plato given out to be copied.

But it is time to bring this discussion to an end. It is needless to examine Professor Burnet's 'bit of *apparatus criticus*' specially prepared for the purpose of this argument. It only shows, when compared with his edition of the *Phaedo*, that his attention has now been directed to four or five of the corrections in question which had not been

previously noticed by himself or by Schanz.

I will only add two remarks which appear to me to be conclusive.

If there had been only one or two corrections of *Clarkianus* written by Arethas and corresponding to the text of his own MS. of Eusebius, they might have been regarded as mere coincidences: but when seven or eight occur on one page, *Clarkianus*, fol. 46^v, in twenty lines of Schanz or Burnet, a proportion far exceeding the average of similar corrections, the probability that they were taken from that MS. is so strong as to amount almost to certainty.

And secondly, if any further confirmation is desired, it may be found in the fortunate circumstance that we are able to test our evidence by a kind of cross-examination.

The same hand which made so many corrections of *Clarkianus* corresponding to the text of Eusebius written by Baanes has also supplied one omission in the text of Baanes from *Clarkianus*.

Of the words $\pi\rho\hat{\omega}\tau\omega\tau\tau\alpha$ (Eus. *P.E.* 26c 1) $\pi\rho\hat{\omega}\tau\omega$ was omitted by the scribe of *Clarkianus*, and $\tau\alpha$ by Baanes. The latter words were supplied in the margin by Arethas (B²) from some older MS., most probably that which was given out to be copied, and the same corrector, B², then proceeded to supply $\pi\rho\hat{\omega}\tau\omega$ in *Clarkianus* in the manner which, by the help of Mr. Madan, I described in the former article.

As an editor of Plato, Professor Burnet can only desire to ascertain the true relations of the various MSS., and, whether he accepts my arguments or not, he must, I think, admit that I have thrown some light on an interesting little episode in the history of the noble MS. which is a pride of the Bodleian Library.

E. H. GIFFORD.

VARIA.

Aeschylus *P.V.* 1030 :

$\omega\dot{\sigma}\ddot{\sigma}$ οὐ πεπλασμένος

ο κόμπος, ἀλλὰ καὶ λίαν εἰρημένος.

λίαν εἰρημένος stands in M as a correction *ex leíain εἰρημένος ut videtur* (Wecklein). If it were not for 1032, it might be possible to defend the words as meaning 'not feigned by me, Hermes, but spoken indeed by Zeus.' As however 1032 goes on

ψευδηγορεῖν γὰρ οὐκ ἐπίσταται στόμα
τὸ Δίον, ἀλλὰ πᾶν ἔπος τελεῖ,

it is plain that πεπλασμένος too must refer to Zeus. Such conjectures as ἀλγήνος and ἐπήτυμος are too unlike εἰρημένος to be plausible: εἰμαρμένος is the best so far. I should like to offer the alternative of ὡρισμένος *fated, settled*.

* * *

Soph. *O.T.* 772:

τῷ γάρ ἀν καὶ μεῖζον
λέξαιμ' ἀν η σοὶ διὰ τύχης τοῦτοῦ λόν;

Is not this a case of the confusion of *μεῖζων* and *ἀμείνων*? See this *Review* xv. 25. No real parallel to *μεῖζον* here is quoted, nor does *μεῖζων* ever mean *preferable* or *more suitable*: on the other hand *καμέίνων* would be just right, to *what better person than you?* i.e. *to what fitter person?* Jocasta has just called herself *δέξια μαθεῖν*, and *ἀμείνων = δέξιατέρα*.

The reverse change seems called for in Lycurgus in *Leocr.* 76 ὅμην γάρ ἔστιν ὄρκος... ἀμνεῖν τῇ πατρίδι καὶ ἀμείνω (read *μεῖζων*) παραδόσειν. The words of the oath, as § 77 purports to give them, are *τὴν πατρίδα δὲ οὐ καὶ ἔλασσον παραδώσω, πλειόν δὲ καὶ ἀμείνω*, where *ἀμείνω* means *stronger*. Cf. also 78 τίνι δὲ ἀν τὴν πατρίδα παρέδωκε μείζω προδοσίᾳ; Make the same correction in Plutarch *Ant. Seni* &c. 786 F τὴν τῶν ταλαιῶν χάριν ἐγείρειν καὶ ποιεῖν ἀμείνω (read *μεῖζων*) καὶ μόνιμον.

* * *

Herodotus 6. 52. When the widow of Aristodemus pretended not to know which of her two infants was the elder, a Messenian advised the authorities to watch and see which she washed and fed first. Thereupon Herodotus tells us that they φυλάξαντας τὴν μητέρα...λαβεῖν κατὰ ταῦτα <αλεῖ Herwerden> τιμέοντας τὸν πρότερον καὶ σίτουσι καὶ λουτροῦσι. What does *πρότερον* mean? The *elder*? That would be absurd, for they did not know which the elder was and were watching to discover. Read τὸν ἔτερον οὐτε. What they found (*λαβεῖν*) was that there was one child whom she always took first. The words are exchanged elsewhere (e.g. Plat. *Theatet.* 163a and 200c, and in *Ar. Nub.* 562 V is said to have *προτέρας* for *ἔτερας*). I should like to make a similar change in an extract from Chamaeleon preserved by Athenaeus 461 B. Chamaeleon is made to say that in Greece οὐτε ἐν γραφαῖς οὐτε ἐπὶ τῶν πρότερον εὑρήσομεν ποτήριον εὐμέγεθες εἰργασμένον πλὴν τῶν ἐπὶ τοῖς ἥρωις, and Kaibel remarks πρότερον *corruptum, requiritur artis operum commemorationis, velut στηλῶν, μημάτων sīm., ἡρίων Wilamowitzi*: but οὐτε ἐπὶ τῶν ἔτερον will give us what we want. Again in Diodorus 3. 59. 3 τὸν μὲν Ἀπόλλωνα φασι..., τὸν δὲ πρότερον..., there seems no meaning in Marsyas being called τὸν πρότερον and we might well read τὸν ἔτερον.

* * *

In Stobaeus *Flor.* 90. 8 three verses are ascribed to Epicharmus, though they have much more the air of the Middle or New Comedy, which run as follows

τινίομ' ὅταν εὐγένειαν οὐδὲν ὀν καλῶς
λέγη τις, αὐτὸς δυσγενῆς ὀν τῷ τραπτῷ
τίς γάρ κατόπτρῳ καὶ τυφλῷ κοινωνίᾳ;

For οὐδὲν ὄν, which is evidently wrong, Ahrens conjectured οὐδὲνῶν, i.e. ἔξουδενῶν, *contemnens*. Apart from any other objection, this seems to give a quite wrong meaning. What we want is that a man shall speak in praise of good birth, though far from well-born himself; that would be like a blind man who insisted on praising, perhaps on having, a looking-glass. For οὐδὲν ὄν I would therefore suggest εὐλογῶν. Nothing is commoner than confusion of ε with ο, and λ with δ: γ and ν are also frequently exchanged (Bast. *Comm.* p. 727). Cf. Eur. *frugm.* 52 περισσόμυθος ὁ λόγος, εὐγένειαν εἰ βρότειον εὐλογήσομεν.

* * *

Menander (Kock 3. 155, Meineke 4. 227) says of man in comparison with other creatures

ἀσθενέστατον γάρ ὄν
φύσει μεγίστους οἰκονομεῖται πράγμασιν
ὅταν πέσῃ δέ, πλείστα συντρίβει καλά,

and for οἰκονομεῖται, which makes no sense, we have such unsatisfactory conjectures as διαπονεῖται and οἰκοδομεῖται. Considering the general sense, may we not feel sure that οἰκο- represents ὄγκο-? ὄγκος is just the word for the superiority of man. Cf. such passages as Eur. *Androm.* 320 μυρίσσι οὐδὲ βροτῶν οὐδὲν γεγόνι βίοισι ὄγκωσας μέγαν. *fragm.* 81 πατενά...λέγειν, ἐς ὄγκον δὲ οὐκ ἄντα βλέπειν τύχης and 825 : Soph. *fragm.* 856 : Herod. 6. 126 ὅτοι σφίσι ταῖτοισι ησαν καὶ πάτρῃ ἐξωγκωμένοι, and very many others. For the correction cf. Cobet's ὄγκον in ἡ πόλις εἰς ἐλάσσονα οἰκον συνήκται (Dionys. *Hal. Ant.* R. 9. 29).

As to the latter part of the compound we cannot be so sure. ὄγκοποιο occurs in the sense required (ὄγκοποιεῖν καὶ ἐπαίρειν τὸν λόγον) in a late writer and ὄγκοποιεῖται may be the word here.

* * *

Diogenes Laertius (1. 104) gives us a saying ascribed to Anacharsis: καὶ τοῦτο ἔφη θαυμασιώτατον ἐρωκάνει ταρπά τοῖς Ἑλλήσιν, ὅτι τὸν μὲν καπνὸν ἐν τοῖς ὄρεσι καταλείποντι, τὰ δὲ ἐντα εἰς τὴν πόλιν κομίζοντι. I can make no sense of καπνόν and conjecture καρπόν. There was a regular Greek

expression *ξύλινος καρπός* *tree-fruit* (see Liddell and Scott, and add Diod. 3. 63. 2: Artem. *Oneir.* 2. 37, p. 133), which we may remember in relation to *ξύλα*.

* * *

Not so easy to set right is a saying of Bion the Borysthenite, also given by Diogenes (4. 48): *τὸ γῆρας ἔλεγεν ὄρμον εἶναι τῶν κακῶν...τὴν δόξαν ἔτῶν μητέρα εἶναι τὸ κάλλος ἀλλότριον ἀγαθόν κ.τ.λ.* It is difficult indeed to see in what sense *δόξα* could be called *a mother of years*. Yet I think a slight addition will give us Bion's real phrase. *αρ* has dropped out after *αν*: what we should read is *τὴν δόξαν ἀρετῶν μητέρα εἶναι*. The sentiment is too familiar to need illustration, but it is expressed briefly and appositely in a fragment of Plutarch (Bernardakis 7, p. 162, *fragm.* 106) οὐδεὶς φροντίζων δόξης ἀγαθῆς γένοιτο ἀνὴρ φαῦλος. Since thinking of this emendation, I have come across the phrase *μῆτρα ἀρετῆς* in Philostr. *Heroic.* p. 667 μὴ τιμῶν ἀλήθειαν, ἦν ἐκεῖνος μητέρα ἀρετῆς ὄνομαζεν εἴωθεν.

* * *

Vita Aeschyli πολλῷ χαλεπώτερον ἦν ἐπὶ Θεσπίδι Φρυνίχῳ τε καὶ Χοιρίλῳ εἰς τοσούνδε μεγέθους τὴν τραγῳδίαν προαγαγεῖν ἢ ἐπ' Αἰσχύλῳ εἰπόντα εἰς τὴν Σοφοκλέους ἐλθεῖν τελειότητα.

In spite of one or two other passages (e.g. *schol. Ar. Ach.* 378) I can not believe that *ἐπίστιν* was thus used of dramatists. Here *ἐπίστιν* is a very obvious conjecture, coming after *Aeschylus*. Twice at least in Plutarch's *Moralia* (*De Pyth.* 405 F and *Adv. Coloten* 1108 F) *ἐπιών* is necessarily corrected to *εἰπών*.

* * *

Platonius *de Diff. Charact.* δὲ Ἐριστοφάνης τὸν μέσον ἀλγάκε τῶν ἀνδρῶν χαρακτῆρα, intermediate, that is, between Cratinus and Eupolis. Though something like *μέσον* or *μέσην* ὁδὸν ἐλαύνειν would be intelligible, I am inclined to think that *μέσον* ἐλαύνειν *χαρακτῆρα* is not right and that for *ἀλγάκε* we should read *ἀξύλωκε*, a word constantly used in these writings.

* * *

Xenophanes *ap. Aristotle Rhet.* 1377a 20 οὐκ ἵστη πρόκλησις αὐτῇ ἀσεβεῖ πρὸς εὐσεβῆ.

By reading *δυσσεβεῖ* for *ἀσεβεῖ* we get a good trochaic tetrameter. So in *Bacchae* 263, where our MSS. give *τῆς εὐσεβείας* and

the *Christus Patiens τῆς ἀσεβείας*, there is no question that Reiske's *τῆς δυσσεβείας* is right.

* * *

Two Pythagorean comparisons (Mullach 1, p. 489), preserved to us by Stobaeus, admit of very obvious correction.

32 οὐτε τὰ τοῦ Ἀχιλλέως ὅπλα τῷ Θερσίτῃ οὐτε τὰ τῆς ψυχῆς ἀγαθὰ τῷ ἄφρον ἀρμόττει.

τῆς ψυχῆς should be *τῆς τύχης*. How can a fool have the goods of the mind? The confusion of *ψυχή*, *τύχη* is quite well known, e.g. Isocr. 12. 9. In a fragment ascribed to Democritus (Mullach 1, p. 341) ἀνοίμονες ῥυτομοῦνται τοῖσι τῆς ψυχῆς κέρδεσσι οἱ δὲ τῶν τοιώνδε δαμονες τοῖσι τῆς σοφίης. I think the correction *τύχης* has been made.

35 οὐτε πῦρ ιματίω περιστεῖλαι δύνατὸν οὐτε αἰσχρὸν ἀμάρτημα χρόνῳ.

The confusion of *χρόνος* and *λόγος* is equally well established (cf. this *Review* xvi. 160). Read *λόγω* for *χρόνῳ*, comparing the saying (Mullach 1, p. 501) τὰ ἀμαρτήματά σου περῶ μὴ λόγοις ἐπικαλύπτειν ἀλλὰ θεραπεύειν ἐλέγχοις.

* * *

Aelian *Var. Hist.* 9. 3 Κλεῖτος δὲ ἐποτε μὲλλοι τοι τρηματίζειν, ἐπὶ πορφυρῶν εἰμάτων βαδίζων τοὺς δεομένους προσίστο.

Certainly Clitus did not come to meet his petitioners, nor did they find him walking about. For *βαδίζων* read *καθίζων*. *β* and *κ* are often confused.

* * *

Posidonius *ap. Athenaeus* 234 A, after mentioning the condemnation of Glyippus for embezzlement, adds τοῦ δὲ ἀνατιθεμένον θεῷ καὶ συγχωρούμενον δήμον καθάπερ κορμήματος καὶ κτίματος (i.e. gold) οὐ ράδιον ἦν τὸν θυητὸν ὀλίγωρον γενέσθαι.

In this passage *δήμον* seems quite unmeaning, and, if it were right, some word like *ἰδίᾳ* would have been introduced later on to balance it as *θυητὸν* balances *θεῷ*. It is probably just a mistake for *δήπον*.

* * *

Plutarch, *De lib. educ.* 2 D οἱ δὲ ἀρμάτειοι τροχοὶ τόνῳ καμφέντες οὐδὲ ἀν εἰ τι γένοιτο τὴν ἔξ ἀρχῆς δύναντ' ἀναλαβεῖν εὐθυνάριαν.

Read *τόνῳ*, comparing *Bacchae* 1066 κυρτὸς τροχὸς τόνῳ γραφομένος (though the meaning there is disputed) &c.

* * *

In the well-known fragment ascribed for a long time to Dicaearchus (Müller, *Frag.*

Gr. Hist. 2. 258. § 14) the writer describes the Thebans as *θρασεῖς καὶ οἰβρισταὶ καὶ ὑπερήφανοι πληκταὶ τε καὶ ἀδιάφοροι πρὸς τάντα ξένον καὶ δημοτρή*, where *ἀδιάφοροι* is understood, I imagine, in the unproved sense of *making no distinction*. No doubt the original word was *εὐδάφοροι* (from *διαφέρεσθαι*) *quarrelsome*. It is needless to illustrate anything so common as this confusion of *εὐ-* and *ἀ-*. I will only point out two cases in which it does not appear to have been corrected hitherto.

Diodorus 13. 23. 4 οὐ γὰρ δυνατὸν τὸν ἄλλοις ἀγνοέρως χρησαμένοντος αὐτὸν παρ' ἔτέρων τυχεῖν τοτε φιλανθρωπίας, ἀλλὰ πράξατος δενά πατεῖν εὐγνώμονα. This *contre-sens* has been dealt with in various ways, but to my mind it is clear that we should read *ἀγνόμονα*.

Again among the fragments above mentioned, collected by Mullach we read (p. 489, 22) ἐν μὲν ταῖς μέθαις παροιοῦσιν, ἐν δὲ ταῖς ἀνυχίαις παρανοῦσιν οἱ ἀνόητοι. Common sense would suggest *εὐνυχίαις*, even if we did not read two pages before (487, 54) ἐν μὲν τοῖς συμποσίοις ὁ μῆτ παροιῶν ἥδιτερος, ἐν δὲ τοῖς ἀγαθοῖς ὁ μῆτ παρανοῶν. But the former passage also gives us a correction of the latter, *παρανοῶν* for *παρανοῦν*. This seems better than reading *παρανομοῦσι* in the former. Foolish men are often intoxicated by good luck, but there is no reason for saying that they proceed to break the law.

* * *

Heliodorus, the paraphrast of the *Nicomachean Ethics*, writing on *Eth.* 8. 5 says (Heylbut p. 169. 37) συνάπτει δὲ τὸν φίλον τὸ τοῖς αὐτοῖς χαίρειν καὶ τὸν αὐτοὺς φιλεῖν. διὰ τοῦτο γὰρ ὁ Σωκράτης φιλεῖ τὸν Πλάτονα, ὅτι τὸν Σωκράτην ἄμφοι φιλοῦσι καὶ τὸν Σωκράτον ἄγαθοῖς ἄμφοι χαίρουσι. Socrates loving Plato because both love Socrates is nonsense, and it is odd that Heylbut did not see this or, seeing it, could not put it right. For ὁ Σωκράτης read ὁ Ιτοκράτης. Cobet in *Novae lectiones* p. 677 mentions two or three instances of the same blunder and has corrected it also in Aelian *V. H.* 2. 36.

* * *

Diodorus 14. 62. 2: διακόσιαι μὲν γὰρ καὶ πεντήκοντα μακρὰν ναῦς εἰσέπλεον . . ., μετὰ δὲ ταῦτα αἱ φορτηγοὶ ναῦς εἰσθεόμεναι μὲν ὑπὲρ τρισχλίας, φέρουσαι δὲ πλεῖστον τῶν πεντακοίων, αἱ δὲ πάσαι σχεδὸν δισχλίαι.

I will not attempt to deal with the arithmetic of this passage, but I think it is possible to correct *εἰσθεόμεναι* with fair

certainty, though Vogel's critical notes offer nothing more satisfactory than 'εἰσέθεον κενὰ Sintenis et Madvig, *εἰς πάλεμον* (vel λυμένα) Wurm, οἰστόμεναι Stroth.' What lurks under it is surely *εἰς ἀριθμόν*, a regular expression for *in number*.

* * *

[Plato] *Ep.* 2. 310 c: εἰ γὰρ ἥρχον ἔγω οὐτῷ τῶν τε ἄλλων καὶ σοῦ καὶ Δίωνος, πλειόν ἦν ὑμῶν τε τάσιν ἀγαθὰ τοῖς τε ἄλλοις Ἐλληνοῖς, ὡς ἔγω φημι νῦν δέ μέγας ἔγω εἰμι διανατὸν παρέχων τῷ ἐμῷ λόγῳ ἐπόμενον.

It was not till I had puzzled many times over the last words that I saw *μέγας* to be a mistake for *μόνος*.

* * *

Lucian (l) *Amores* 44, speaking of an ordinary boy's life, describes how he gets up and goes out in the morning τὴν ἵεραν χλαμύδα ταῖς ἐπωμίοις περόναις συρράγεις. Why should his chlamys be called *ἵεραν*? What it no doubt really was is *ἔρεα*, *woollen*, as Plato *Crat.* 389 b speaks of a *ἱμάτιον λαοῦ* ἡ *ἔρεον* and in *Polit.* more than once of *ἔσθῆς ἔρεα*.

* * *

The dictum of Protagoras about the gods appears in many places in slightly different words. In modern books it is most often given as in Diogenes 9. 51: περὶ μὲν θεῶν οὐκ ἔχω εἰδέναι οὐθὲν ὡς εἰσὶν οὐθὲν οὐκ εἰσὶν. A number of other forms, not necessarily purporting to be verbally faithful, will be found in Mullach 2. 131. In the case of so famous a saying we should like to make quite sure of the actual words. This is, I fear, impossible; but has any one pointed out that οὐκ ἔχω εἰδέναι, which is the form in Suidas too, is very unlikely to be right? οὐκ ἔχω εἰπεῖν, though apparently not given by any authority, is much more the expression Protagoras would use. *Εἰπεῖν* or some such word follows *ἔχω* and *οὐκ ἔχω* in places innumerable; but it would probably be impossible to find *εἰδέναι* after either of them. Indeed οὐκ *ἔχω* itself is very often roughly identical with οὐδὲν, as in οὐκ *ἔχω τις ἀγενοίμαν* (Aesch. *P. V.* 904), and even the common οὐκ *ἔχω* οὐ τι λέγω. What he probably meant, as we should, was *I cannot say*, not *I cannot know*; and, if he had really meant *I cannot know*, he would probably have used another word or words, not *ἔχω*. Though not impossible then, οὐκ *ἔχω εἰδέναι* is very improbable. Four quotations of the Greek, given by Mullach, have οὐκ οὐδὲν, two οὐ δύναμαι λέγειν. Cicero's Latin version in *N.D.*

I will not attempt to deal with the arithmetic of this passage, but I think it is possible to correct *εἰσθεόμεναι* with fair

1. 23. 63, which is really our earliest authority, *de divis neque ut sint neque ut non sint habeo dicere*, suggests *οὐκ ἔχω εἰπεῖν*. He says also (*ib.* 12. 29) *Protagoras, qui sese negat omnino de divis habere quod liqueat*,

but this does not purport to be more than an equivalent. Perhaps then the evidence as a whole points to *οὐκ ἔχω εἰπεῖν*, of which *εἰδέναι* may very well be a corruption.

HERBERT RICHARDS.

ON SOME GREEK COMPARATIVES.

SINCE Johannes Schmidt, *K.Z.* xxv. 156, explained ἐλάσσων, θάσσων from *ελαγχίων, *θαγχίων, that has been the generally accepted account of the forms. Some, e.g. Brugmann, *Grundriss* I² 363, *Kurze Vgl. Gramm.* 113, Hoffmann, *Gr. Dial.* III. 312, suppose the development to have been *ελαγχίων > *ελανσσων > ἐλάσσων. Others suppose that *ελανσσων became *ελασσων, *ελάσσων and that σσ came analogically from other comparatives; so G. Meyer, *Griech. Gramm.*³ 488. To both of these explanations serious objections have been raised by Lagercrantz, *Zur griechischen Lautgeschichte*, pp. 33 seqq. Unfortunately there seem to be no examples of νκί, νχί except the comparatives in question, so that there can be no absolute demonstration of the impossibility of the alleged change, but it is not in accordance with what takes place in similar combinations in Greek. As to the alternative explanation, the comparatives in which -σσων would have developed regularly are in a great minority, in ordinary Attic there is only ἥττων, so that *-σσων might have been expected to prevail rather than -σσων, -ττων.

There is another difficulty still. As has been pointed out by Blass, *Kühner-Blass, Griech. Gramm.* I. 555 and by Lagercrantz, *op. cit.* 35 seqq., there seems to have been in such comparatives a difference of quantity between Attic and other dialects. The matter rests on a passage of Herodian:—(I. 523, 29, cf. II. 13, 13. 942, 17) τὰ εἰς στον συγκριτικὸν οὐδέποτε ἐκτεταμένη διχρόνω παραλήγεται, βράσσων, πάσσων, μάσσων, γλύσσων, βάσσων, ἐνθει παρ' Ἐπιχάρμῳ βάσσον, τὸ ἐλάσσων, θάσσων “θάσσονας ἱρικῶν ἔμεναι καλλιτριχας ἵππον” σειρημέναι, ὅτι φθορει μακρὰ παραλήγεται. As Lagercrantz remarks, it were indeed strange that a should be long in θάσσων, short in πάσσων. As Blass and Lagercrantz have shewn, θάσσων has good authority in Homer; of θάσσων there are not many instances (Lagercrantz, *op. cit.* p. 38), but, as this accentuation is against the precepts of the grammarians, the ex-

amples are not to be lightly set aside. All this points to the fact that the Attic quantity was different from that of other dialects. Herodian's statement about ἐλάσσων and θάσσων has been explained very simply by Blass and Lagercrantz; they are the only two of these comparatives that are found in the Attic dialect. In support of such a quantitative difference may be cited further Att. κρέττων, μεζών, which have been rightly explained by Brugmann as analogical lengthenings of *κρέττων, μεζών. If the other comparatives differed in quantity, it is easily intelligible why this lengthening took place in Att. and not in other dialects: Ion. κρέσσων, μέζων, Arcad. μέζων, μέττων μεζόν Hesych., probably Cretan.

How then are these comparatives with ă to be explained? It will be observed that where a weak form of the root appears in the positive and the superlative in Greek it likewise extends to the comparative:—βαθύς βάσσων βάθυσσων, βραχύς βράσσων (Aeol. βρόσσων) βράχυσσων, γλυκύς γλύσσων γλύκυσσων. In accordance with that ἐλάσσων, θάσσων, πάσσων may be explained without difficulty from *ελαχίων, etc. Lagercrantz apparently takes another view, for he derives θάσσων from *ανσσων. But of such a change there is no other instance, and it is unnecessary to postulate it here. θάσσων may simply have followed the analogy of the other comparatives in -σσων. Dialectically δλεῖσσων¹ fell a victim to δλίγος δλίγυστος and became δλίζων; so in Cret. *κρέττων became κάρτων from κάρτιστος. Ion. ἥσσων (Arcad. ἥσσων) may be put down to the influence of the opposite κρέσσων. Corresponding to Att. μάλλον the Ionic was probably μάλλον (cf. μάλα μαλλάτο).

What then of the long vowel in Attic?

¹ As might be expected from δλίγος and δλίγυστος, ει of δλεῖσσων was the genuine diphthong, cf. Schwyzer-Meisterhans, *Grammatik der attischen Inschriften*, pp. 36, 52, 151. The spelling δλεῖσσων is like πεσιδος, κεται etc., *ib.* p. 36.

That these comparatives were formed differently in Attic is very improbable. Lagercrantz has tried to show that in Attic a short vowel was lengthened before $\tau\tau < \kappa\chi$, $\chi\iota$ and before $\zeta < \gamma\iota$. But as Thumb, *Idg. Anz.* xii. 63, has pointed out, this alleged law rests on the comparatives in question, for $\mu\ddot{\alpha}\zeta\alpha$ admits of another explanation, and against it are words like $\delta\tau\alpha$. A simple explanation offers itself if we consider together all the Attic comparatives in $-ov$. Many of them had regularly a long vowel or a diphthong:— $\bar{\alpha}μείνων$, $\eta\tau\tauων$, $\delta\lambda\epsilon\zeta\omegaν$, $\pi\lambda\epsilon\omegaν$, $\chi\epsilon\iota\omegaν$ (Aeol. $\chi\epsilon\iota\omegaν$). Over against these stand $\delta\lambda\alpha\tau\tauων$, $\theta\alpha\tau\tauων$, $\mu\ddot{\alpha}\lambda\lambda\omegaν$, $\kappa\tau\epsilon\iota\tau\tauων$, $\mu\ddot{\epsilon}\zeta\omegaν$. Now, with the exception of $\theta\alpha\tau\tauων$, these comparatives belong to the same sphere of meaning. Thus *κρείττων* is the opposite

of $\eta\tau\tauων$, $\delta\lambda\epsilon\zeta\omegaν$ of $\mu\ddot{\epsilon}\zeta\omegaν$, $\delta\lambda\alpha\tau\tauων$ of $\mu\ddot{\epsilon}\zeta\omegaν$ and $\pi\lambda\epsilon\omegaν$. Thus it is little wonder that the vocalism of the latter class should have been influenced by that of the former. How the analogy operated in detail cannot be made out with certainty. *κρείττων* (*i.e.* *κρέττων*) would naturally be influenced by the opposite $\eta\tau\tauων$; $\mu\ddot{\epsilon}\zeta\omegaν$ may have been influenced by $\delta\lambda\epsilon\zeta\omegaν$, but I have no certain evidence to show whether ϵ in $\mu\ddot{\epsilon}\zeta\omegaν$ represented *ei* or *ɛ*; $\delta\lambda\alpha\tau\tauων$ may have been influenced by $\eta\tau\tauων$ of similar meaning or by its opposites; $\mu\ddot{\alpha}\lambda\lambda\omegaν$ is the opposite of $\eta\tau\tauων$. As for $\theta\alpha\tau\tauων$, when $*\delta\lambda\alpha\tau\tauων$ had become $\delta\lambda\alpha\tau\tauων$, $*\theta\alpha\tau\tauων$ would become $\theta\alpha\tau\tauων$ under its influence:— $\tau\alpha\chi\delta\zeta$: $\theta\alpha\tau\tauων$: $\tau\alpha\chi\iota\omega\tau\omegaν$ = $\delta\lambda\alpha\tau\tauων$: $\delta\lambda\alpha\tau\tauων$: $\delta\lambda\alpha\chi\iota\omega\tau\omegaν$.

J. STEACHAN.

ON THE FIRST ODE OF HORACE.

ENOUGH has been written on the first Ode of Horace to make one hesitate before adding to that *ἄχθος ἀρούρης*. It is only a firm conviction, based on much careful study and thought, that the first Ode as printed and pointed in the current editions is but a travesty of Horace that has impelled me to the present writing.

It is the plain duty of an interpreter of this poem to answer two plain questions: (1) What is Horace driving at? (2) How did he say what he had to say? These two questions and their answers are closely bound up together; but I will try to maintain such separation as the case admits of. Let us see then first what Horace is driving at.

The poem may be divided in several ways. For the purposes of our immediate enquiry it may be divided into a personal part consisting of vv. 1—2 and vv. 29—36 and a non-personal part consisting of vv. 3—28. The gist of the non-personal part (vv. 3—28) is plain (or ought to be so) to an attentive reader. What is it? It might be hastily said that it is an elaboration of the theme: *Trahit sua quemque voluptas*. But that is worse than false: it is only a half-truth. The real theme is: *Trahit sua quemque voluptas, cui voluptati aliena semper opponitur voluptas*. We have in these verses three contrasts of pursuits of men, the first in vv. 3—10, the second in vv. 11—18, the third in vv. 19—28. In the first division (vv. 3—10) the favourite

pursuits of nations are contrasted, in the two other divisions (vv. 11—18 and 19—28) the pursuits of various classes of men are contrasted. But before going further on this line I must take up the question of the punctuation and interpretation of vv. 3—6.

In vv. 11—18 the reference to the farmer and the reference to the skipper are set off sharply and neatly the one against the other. The *gaudentem* at the head of v. 11 is balanced in form (though not in sense) with the *luctantem* at the head of v. 15. It is significant that it is the latter and not the former term that but formally suberves the balance aimed at. In vv. 19—28 we find the *est qui* at the head of v. 19 answered by the *multos* at the head of v. 23. And this brings us around to the *sunt quos* of v. 3. What answers to it? Surely not the *hunc* and *illum* of vv. 7 and 9: these words merely introduce subdivisions like *τὸν μὲν . . . τὸν δὲ*. No; *sunt quos* must be answered by something in v. 6, and that something is clearly *terrarum dominos* = *Romanos*, whether Horace had Virgil's happy phrase in his head or not. All this seems so plain that it fairly makes one rub his eyes to find the latest editors ignoring it. The late Lucian Mueller to be sure puts 'mit dem feinsinnigen J. Rutgers' a; after *nobilis* (v. 5). But—mirabile dictu—although he writes of v. 6 as a whole—what were better written of *terrarum dominos* alone—, 'mit Emphase an den Anfang gesetzt, im Gegensatz zu *sunt*

quos—iuvat,' he yet takes *terrarum dominos* as appositive to *deos*. But perhaps the good punctuation may outweigh the bad annotation.

Perhaps I may properly suggest at this point that in vv. 3—5 Horace—primarily, it should seem, for reasons of metre—did not continue the construction of *collegisse* formally but merely informally. What he wrote, however, I venture to think equivalent to *sunt quos curriculo pulverem Olympicum collegisse iuvat metamque fervidis evitasse rotis palmaque nobilitatos fuisse*.

To return now to our interrupted discussion of the general meaning of the poem, we have in vv. 3—10 the pursuits of nations (Greeks on the one hand, Romans on the other) contrasted. In vv. 11—28 we have contrasted the pursuits of various classes of men; in vv. 11—18 we find farmer and skipper, in vv. 19—28 the man of ease and leisure and those that follow the strenuous life, whether in war (vv. 23—25) or in the chase (vv. 25—28).

But how is all this, the non-personal part of the poem, connected with the rest, the personal part? It could only be, it should seem, by joining on to the non-personal contrasts a personal one, *i.e.*, inasmuch as the poem is addressed by Horace to Maecenas, a contrast between Maecenas and Horace. But in the traditional text we have nothing of the sort. *Me doctarum hederae* etc. follows what we have been discussing just as if the matter had all been after all a little sermon on the text 'Trahit sua quemque voluptas'. But that that is not the burden of Horace's song has been clearly set forth above. Are we not then led to look with more favour upon, nay even to accept, the old conjecture *te* for *me* in v. 29, *detestatum editoribus* though it be? Modern Horatians might, I venture to think, be about worse business than reading, marking, and inwardly digesting Wolf's *Commentatio ad Hor. Carm. I. 1, 29* (*Litterarische Analekten* II, 261—276). Orelli to be sure hints that Wolf may not have been in earnest; but perhaps there was something wrong with Orelli's sense of humour. Wolf was not playing a practical joke; and it is needless to say that what he wrote is well written, whatever you may think of the view he takes. To me at least the *Commentatio* is convincing, although I do not think that Wolf made all he might have of his case. I have advanced above in favour of *te* an argument from the contrasts in the ode that he did not make use of.

But he has argued well in favour of the need of a reference to Maecenas at the close of the ode in a more pointed form than that which the traditional text presents. Indeed, does not a very recent editor (as others had done before him) comment on the lack of a *tu* in v. 35? Wolf might indeed have said plainly (what is a fact) that this whole poem is just one long sentence and that the only place where you can put a full stop without spoiling it is at the end of v. 36.

I wish to set down this poem in full in the form that I believe Horace meant it to bear and with a rational scheme of punctuation, but before doing that I would lay before the reader a set of notes lately drawn up in which some of the points discussed above are resumed and other matter pertaining to the division of the ode is included.

(1) The ode deals not with the simple theme *Trahit sua quemque voluptas* but with the complex theme *Trahit sua quemque voluptas, cui voluptati aliena voluptas semper est opposita*.

(2) If the theme were the former of the two just mentioned, such a climax as *me . . . me . . .* would be natural; but inasmuch as the theme is the latter, the climax should be itself a contrast of terms.

(3) There is throughout the ode a regular series of contrasts—nation contrasted with nation (Greeks x Romans), class of men contrasted with class of men (*agricola* x *mercator*, *desidiosus* x *strenuus*—the strenui being represented by two classes: (a) *milites*, (b) *venatores*), individuals contrasted (Maecenas x Horace).

(4) The divisions of the ode should be observed. These are: (a) Address to Maecenas (2 vv.) + Greeks and Romans (3 + 5 vv. = 8 vv.) = 10 vv.

(b) Farmer (4 vv.) and skipper (4 vv.) = 8 vv.

(c) Man of ease (4 vv.) and men of action (6 vv., of which the last two might be dispensed with without detriment to the sense) = 10 vv.

(d) Maecenas the lofty poet (1½ vv.) and Horace the humble poet (4½ vv.), to which is added the climax and conclusion (2 vv.) = 8 vv.

It will thus be seen that the poem falls into two divisions of 18 vv. each and that these divisions are severally subdivided into a group of 10 vv. and a group of 8 vv. Furthermore, the first group of 10 vv. is balanced with the second group of 8 vv.; contrasted nations are balanced with contrasted individuals; the first two verses (1—2) are balanced with the last two

(35—36). Again the first group of 8 vv., which deals with classes of men, is balanced with the second group of 10 vv., which deals with classes of men, these two groups forming the centre and core of the poem. The whole scheme may be represented thus :

$$\left(\begin{array}{l} \{ 10 (= 2 + 8) \\ \quad 8 \\ \hline \quad 10 \\ \{ 8 (= 6 + 2) \end{array} \right)$$

When we have grasped this arrangement we may perhaps see why vv. 27—28 were added to vv. 19—26 : it was that the scheme of the second part of the poem (10 + 8) might match that of the first part (10 + 8). Is it going too far to think that this poem was built upon and around vv. 11—26 ? This question I shall take up presently : following is the text.

Maeccas atavis edite regibus,
o et praesidium et dulce decus meum,
sunt quos curriculo pulverem Olympicum
collegisse iuvat metaque fervidis
evitata rotis palmaque nobilis ;
terrarium dominos evicit ad deos
hunc si mobilius turba Quiritium
certat tergeminis tollere honoribus,
illum si proprio condidit horreo
quidquid de Libycis verritur areis :
gaudentem patrios fidens sarculo
agros Attalicis condicibus
numquam demoveas ut trabe Cypria
Myrtoum pavidus nauta secat mare ;
luctantem Icaris fluitibus Africum
mercator metuens otium et oppidi
laudat rura sui, mox reficit ratis
quassas indocilis pauperiem pati :
est qui nec veteris pocula Massici
nec partem solidu demere de die
spernit, nunc viridi membra sub arbuto
stratus, nunc ad aquae lene caput sacrae ;
multos castra iuvant et lituo tubae
permixtus sonitus bellaque matribus
detestata ; manet sub Iove frigido
venator tenerae coniugis immemor,
seu visa est catulis cerva fidelibus,
seu rupit teretes Marsus aper plagas :
te doctrum hederae praemia frontium
dix miscent superis ; me gelidum nemus
Nympharumque leves cum Satyris choi
secernunt populo—si neque tibias
Eutripe cohobet nec Polyhymnia
Lesboum refugit tendere barbiton—,
quod si me lyricis vatibus inseres,
sublimis feriam sidera vertice.

Returning to the question broached above I may note that Lucian Mueller calls attention in his massive edition of the Odes (I, p. 131) to a similarity (a "merkwürdige Aehnlichkeit" he calls it) between this Ode and the first Satire. That similarity is, I venture to think, to be traced in the Ode in just one place, viz. vv. 15—18, and it

consists in the fact that the mercator while on the sea in bad weather wishes himself snug at home in his native village. He is in so far discontented with his lot and laudat diversa sequentis. Now this is in the part of the first Ode about which it has been queried above whether it were not the nucleus of the whole composition, and that fact may afford us some reason for assenting to that view. Vv. 11—18 are very symmetrically arranged, more so than any other part of the poem, and in thought they are most closely connected with vv. 19—26 (to which latter verses it has been suggested above that Horace added vv. 27 and 28 *et devrēpas φροντίδος*). Vv. 3—11 are not so symmetrical but make up the rest of the non-personal part of the poem. In short, whether we do or do not assume, and it is, of course, mere matter of curious speculation, that vv. 11—26 are the original nucleus of the poem, it seems tolerably clear that a good deal of the difficulty in the interpretation of the poem is due to the grafting upon an originally impersonal poem of a personal poem, the personal part consisting clearly of vv. 1—2 and vv. 29—36. That the personal part was grafted on the impersonal and not vice versa seems fairly clear from the fact that vv. 1—2 + vv. 29—36 make up a total of 10 verses, a number not divisible by 4. On the other hand, vv. 3—26 are = 24 verses = 6×4 . Thus working from the point of view of the stanza and not, as above, from the point of view of the divisions of the poem according to sense, we can see a reason why vv. 27—28—verses so eminently Horatian that their authorship ought never to have been called in question—have been added, viz. to round out the number 36. I am willing to risk the charge of inconsistency that my double demonstration (if demonstration it be) lays me open to.

One more point. With *te* for *me* in v. 29 there is a clear contrast between *dis miscent superis* in v. 30 and *secernunt populo* (= profano vulgo) in v. 32. Again, the *quod si* of v. 35 means 'but if' and introduces a clause contrasted with *me gelidum nemus . . . populo* (the *si neque . . . nec . . .* clause is a mere parenthetical proviso = *modo . . . cohobat . . .*). But if this be so, the last verse must have a different meaning from that usually assigned to it and will refer not to Horace's pride but to his fame and dignity. Perhaps, it may be added, we should in v. 35 write rather *Lyricis Vatibus*.

I have written on a very interesting subject at greater length than I had

expected to; but perhaps I may be forgiven for adding in closing a quotation and suggesting a query. In the English-Latin edition of Horace published in London in 1750 ('Begun by David Watson, M.A. of St. Leonard's College, St. Andrew's; Revised, Carried on, and Published by S. Patrick, LL.D.'), we read in the 'Key' to the first Ode :

'After he has shewn, that every Man has a different choice, according to his own peculiar Will and Fancy, in the pursuit of Happiness here, he complimentis his Patron in these Words: *Hederae corona ex hedera, quae sunt præmia doctrarum frontium, miscent te, meum Patronum & Fautorem, Dis superis.* The Ivy, or Crown of Ivy, the Reward of learned Men, rank you, my Patron and Supporter, among the Gods above. As for himself, he, as all Men of good Sense and Education, keeps himself at a distance from his *Macenas*, saying, *Gelidum nemus & leves chorii nympharum cum satyris secernunt me a populo.* The cool Grove and light Choirs of *Nymphs* with the *Satys* separate me from the Vulgar. If the Muses *Euterpe* and *Polyhymnia* will hear my Invocations, and you *Macenas*, patronize my Compositions, *feriam sidera sublimi vertice,* I shall touch the Stars with the top of my Head, shall reach at the highest pitch of Fame, which will last forever.—That this is Horace's Meaning, is plain from ODE XXX. Book III. where he says, &c.'

In the corresponding place in the 'Annotations' we read :

'All the Editions of *Horace* had *Me* formerly, till of late it was observed by the Right Reverend Dr. *Hard* Bishop of *Chichester*, that if *Horace* wrote *me*, he had no need to wish for a cool Grove, for the Company of the *Nymphs* and *Satys*, to be ranked among the Lyric Poets, and to touch the Stars with the Top of his Head, when he was already among the Gods above, eating and drinking *Ambrosia* and *Nectar*. We must suppose therefore that *Horace* is not speaking of himself, but complimenting his Patron *Macenas*, in saying that the *Ivy-Crown*, the Reward of learned Men, exalted him among the Gods above. We are not to imagine *Horace* was so ignorant of the way of complimenting his Patron, as to prefer himself to him, upon whom he was in Gratitude passing a Compliment.'

This and more to the same purport : the date (see the end of the 'Key' to Ode I) 'this present year of our Blessed Lord 1739'. Let the reader—that 'gentle' and 'indulgent' creature of an earlier day—now open one of the latest modern editions, read text and notes, and ask himself the question : Is interpretation a failure, or are the Horatians played out ?

MORTIMER LAMSON EARLE.

18 November, 1901.

THE CLUNI CODEX OF CICERO.

In his very welcome and highly appreciative review of my recent volume in the *Anecdota Oxoniensia* series, Mr. Clark is seemingly only half-convinced of the truth of my thesis, that the 15th century codex known as *Lagomarsini* 42 was directly copied, for the Second and Third Book of the *Verrines*, from the Cluni manuscript. As further research has put me in possession of fresh evidence, I beg leave to submit it. The importance of the subject must be my excuse for returning to it. The identification of the mutilated codex now at Holkham with the famous 'liber Nannii,' as well as with the so-called *Fabricianus* and the *Metellianus* (see Baiter-Halm, vol. ii. p. 177) would be of little more than merely antiquarian interest at the present day were it not for the fact that we may confidently accept Lg. 42 as a fairly accurate transcript, before its mutilation, of the latter part of the manuscript whose remarkable history I have endeavoured to establish.

No one has given more attention to extant MSS. of Cicero than Mr. Clark, and if I

can secure his unqualified adhesion to my views, I shall feel that the case is in a fair way of being closed. I cannot wonder that so wholesale and far-reaching an identification as that which I have proposed should have been in some quarters received with a certain degree of incredulity; but when Professor Robinson Ellis argues that if the codex used by Nannius, and Fabricius, and Metellus had been of the 9th century, one or other of them would probably have termed it *vetustissimus*, he appears to overlook the fact that it was obviously on the authority of Fabricius that this very epithet is applied to it by Lambinus, who used the collation Fabricius had made (see *Anedoton*, p. xl). And *v.c.* is the abbreviation constantly employed by Nannius for 'vetus codex.'

We are more scientific in our methods of collating now than Nannius was, but wrong reporting is still a fruitful source of error. To give a simple instance. If Lg. 42 had originally the word *iudices* in the first line of the Second Book of *Actio* ii, a prima

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facie case might have seemed to exist against the view that it was copied directly from the Cluni codex, where *iudices* certainly does not occur; and I was therefore somewhat disturbed when Mr. Clark brought forward this discrepancy, stating that in Lg. 42 *iudices* stands as part of the text. But on turning to the Zürich edition (p. 451) for a verification of the statement I had made (Anecdoton, p. xxvii), I find Halm's note quite clearly stating that *iudices* is omitted by the first hand in Lg. 42,—which is sufficient for my argument. We know that the *manus prima* was corrected—sometimes by the writer himself—from the deteriorios; but all the slight discrepancies between it and the text of the Cluni codex as we have it now, or as we can reconstruct it from the *Nannianus*, the *Fabricianus*, and the *Metellianus*, are just such as might arise in the course of a somewhat hasty transcript, made in the 15th century by a scholar who had doubtless some other knowledge of the text. No weighty argument can be founded on the fact that the writer of Lg. 42 often prefers a form different from that which he found in his original (e.g. *omnes* for *omnis*, *ab* for *a*, *imperi* for *imperi*, *apponit* for *adponit*): or on such misreadings as *nemo* for *venio*, and *domi* for *dein*: or even on such a rare aberration as *obedire* for *abdite* (Müller, p. 265, 26). If he even varied, at times, the order of words in his original, he was only following the prevailing fashion of copyists, who had their preferences, and who liked to give, in this way, a little touch of individuality to the copies they made. The element of unconscious transposition must also, of course, be allowed for.¹ As to the

¹ Some at least of the transposition variants which appear in Lg. 42, as against all the other codd., will not be reproduced in my forthcoming edition of the Verrines. C. F. W. Müller seems to me to have accepted too many of them. The process out of which they must sometimes, at least, have resulted is revealed, e.g. at p. 343, 2, where all the MSS. have *mihi videtur*, except Lg. 42, which gives *videtur mihi videtur*. Here it would seem as if the writer of Lg. 42 had wanted to change the sequence, but carelessly repeated *videtur*. In the same way, at p. 283, 30, Lg. 42 gives *et quae tuos simus*, where *-que* is the writer's own, inserted either through inadvertence or preference, or after comparison with the dett., while he forgot to delete the preceding *et*. Though Müller has been somewhat inconsistent in dealing with these variants, he has the credit of being the first editor of the Verrines who had a proper appreciation of the value of the tradition contained in Lg. 42, for the Second and Third Books. Here are some of the places where he seems to defer too greatly to its authority. At p. 282, 37, he prints *Quid reliqui est* with Lg. 42 against the codd. and edd. But here the *Fabricianus* is cited for *Quid est*

erasures in C—I gladly accept Mr. Clark's designation of the Holkham codex, instead of the more cumbersome Ho—it is probable that the occurrence of these marks in his original incited the writer of Lg. 42 to such easy emendations as *discederent* for *discrederent* 208, 2, *classibusque* for *classique* 257, 33, and other changes which might appear to raise a difficulty now, even if he could no longer always read the erasure in the text before him.

Against such trivial discrepancies, more of which might readily be cited, I beg the reader to weigh the following: First I shall quote four passages, following the paging of Müller's text, where an almost equal number of words is omitted from the text of Lg. 42:—

p. 312, 30 quod ipsi Leontini publice non sa[n]e
p. 324, 12 non perseveras non perquisis
p. 334, 19 ea monet alienum hominem quae
p. 348, 14 secuti sunt avariores magistratus.

Any one who will take the trouble to

reliqui, which must therefore have stood in *Cluni*, the inversion being probably due to caprice on the part of the writer of Lg. 42. Similarly at p. 282, 21, Lg. 42 has *re vera quidem* for *re quidem vera*: *sociorum contra salutem*, p. 357, 13, for *contra sociorum salutem*: and *acepta pecunia*, p. 228, 20, for *pecunia accepta*. It may well be doubted, therefore, whether the authority of Lg. 42 is sufficient for such a change in the text as *rusticarum rerum*, p. 279, 37, for *rerum rusticarum* (all other codd., including the *Vaticanus*): or *sunt duae*, p. 275, 22, for *duae sunt*: or *sapientiam maiorum*, ibid. 28, for *maiorum sapientiam*: or *vita vitaque*, p. 274, 35, for *vita vitaque*: or *quam sibi*, p. 281, 15, for *sibi quam*: or *decumanum putatis*, ibid. 3, for *putatis decumanum*. All these, however, Müller adopts, though on the same principle he ought to have accepted the inversion in Lg. 42, *praesertim magno*, p. 296, 28, for the vulgate *magnu[m] praeſertim*, and *ut ex maxima parte ut*. The Cluni codex, where it is still extant, may be brought as a witness against the copyist of Lg. 42 in at least two places, which may be cited to complete this note. At p. 209, 5, it gives *nostros comites*, with most codd., whereas the first hand in Lg. 42 has *comites nostros* (wrongly adopted by Müller): here Lg. 42 first capriciously changes the order in C, and then either the writer himself, or the second hand, finding that the dett. were against him, restores the right order. Again, at p. 240, 24, C gives *dies festi aguntur*: this appears in Lg. 42 as *festi dies aguntur*, subsequently corrected. That the first hand himself made corrections in what he had written, on a comparison, probably, with some member of the dett., is obvious from such examples as p. 204, 33, where the original order, copied from C, *negotiantur in Sicilia*, is altered by the m. p. to *in Sicilia negotiantur*.

Two places may be noted on which a suggestion might be based that the writer of Lg. 42 did part, at least, of his work to dictation: p. 241, 22, where he gives *venisset redditus* for *venisset hereditas*, and *ibid.* 25, *propetisset* for *poposisset*.

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look at the facsimile reproduced at p. 11 of my collation, will see for himself that each of the above is of the average length of a line in the Cluni codex. The inference is irresistible: Lg. 42 was directly copied from C, and at each of the places cited above the copyist inadvertently dropped a line.

Similar lacunae elsewhere may be attributed to the common occurrence of omission *per paralepsiam*, of which Jordan quotes four cases within a few lines in a note on p. 314, 15 of the Zürich edition: cp. Müller p. 229, 2: 335, 18: 336, 35: 314, 35.¹ But no such explanation will fit the above passages. It is always possible, of course, to assume that an intermediate manuscript may have existed, and that Lg. 42 is a copy of a copy of Cluni,—the omissions above cited having originally occurred in the first copy. This supposition must, however, be excluded by various passages in which it is obvious that the writer of Lg. 42 is endeavouring to decipher an ancient original, whose readings were not always easy or obvious. I begin with a passage which will connect my argument with what I have to say later on the *Codex Nannianus*, i.e. the Cluni MS. At p. 294, 28, where C is no longer extant, the received reading *nemo abs te productus* has been recovered from Nonius: in Lg. 42 we find *nemo pro opte abductus*: while Nannius read the original (rather differently) as *nemo prompte abductus*.² At p. 291, 23 Nannius reports *M. Cossutius* (a correct citation from C which, as it was at once received into the text, it was unnecessary for either Fabricius or Metellus to repeat): the copyist of Lg. 42 was not so successful, and writes in *eos sutius*. The received text at p. 214, 8-10 is *commonefaciunt ut, si sibi videatur, utatur instituto suo nec cogat* etc.: here Lg. 42 has (according to Reifferscheid-Müller) *commone faciunt ne sibi videaret niti tuo suo ne cogat*, and it may be noted that *sibi* (for the vulgate *ei*) is reported also from the *Metellianus* (i.e. the Cluni codex), which moreover

¹ In the last-named passage the fact that Müller now reports that the lacuna following *Nam cum a multis* includes the *autem* after the second *multis* might lead one to suspect that the *autem* formed no part of the Cluni text. Other omissions, per incuriam, in Lg. 42, should not be considered of any account except where the support of F M or φ shows that they were omissions also in C. Thus I regard the absence in Lg. 42 of *et fortissimo* at p. 308, 16, as accidental: and the same holds of *omnium* (bracketed by Kayser and Müller) at p. 274, 33.

² Nannius carelessly left the first words of his lemma stand uncorrected, and pays the penalty in Jordan's note 'non nescit N.'

contributes *intituo* for Lg.'s less successful *niti tuo*. Gruter read *ad horam viii* at p. 232, 36 for the vulgate *ad horam octavam*, and his reading probably comes from the *Metellianus*: in Lg. 42 the Cluni text appears as *ad bonō viii*. At 236, 22 Lg. 42 gives *possidens i agros alienenses*, which is far enough from the received text, *Posides Macro Soluntius*: and at 306, 20 the equally unintelligible *lignō* for *gloriatur non*. Halm reports *ut his uisis quidem* from Lg. 42 at p. 260, 12 for *ut iis ne in suis quidem*. At 266, 8 (where we have a fragment of C) the *homini* of the original is said to appear as *banum* in Lg. 42: *dormitabat aliquando librarius!* So at p. 334, 16 *insinuet* (or what stood for it in Cluni) is copied as *iusuuri et*, or *uisuuri et*. Such forms would never have been reproduced from any contemporary manuscript: the writer of Lg. 42 knew enough to be aware that where he could not make out the meaning his best course was to copy as nearly as possible what stood in his original. At 345, 5 Lg. 42 is reported as reading *fullū quā cogitassēt et* (per compend.) *hoc a me postulassēt* where the text now gives *si illi umquam hoc a me postulassent, si umquam omnino cogitassent*. I am disposed to think that Cluni here had *si illi umquam cogitassent et hoc a me postulassent*: if the writer of Lg. 42 had been transposing arbitrarily, he would surely have taken the trouble to write more correctly. At p. 355, 18, for *cum in crimen maximo dici a defensore*, Lg. 42 gives *cum morum maximo decio defensore*. The last instance I shall quote of Lg.'s failure to decipher C correctly is at p. 288, 15, where instead of *labefactat enim vehementer* we find *labeſcā enim erat vehementer*. Here the writer of Lg. 42 may again have been making a transposition: his original may have stood *labeſcā rat enim vehementer*.²

So much for Lg. 42. When I published my account of the Cluni codex, I felt quite confident that Lg. 42 was a direct copy: I was more diffident about the identification of the famous 'liber Nannii' with the codex now at Holkham,—mainly on the ground, I fancy, that it was enough to have identified it with the *Fabricianus* and the *Metellianus*. There is a proverb about the love that poets bear to their own fantasies, and Mr. Clark's Harl. 2682 has been humorously spoken of in Germany as 'Clark's

² Reifferscheid's collation of Lg. 42, which was placed at C. W. Müller's disposal, has not been published in full, but it enabled Müller to correct many points in the previous reports of Jordan and Halm.

Liebling H'! But I can now confirm abundantly what is stated, somewhat diffidently, in my *Anecdoton*. The Cluni codex was certainly, while still complete, in the hands of Pierre Nanning (1500-1557)—probably some time after it had been copied by the writer of Lg. 42. But the Dutch scholar's method of reporting the results of his collation would certainly not commend itself now. In each case he first cites, in his lemma, the reading of the vulgate, as it was at his day. Then in his note he embodies the particular correction which he wished to report from C, and either leaves the rest of his citation as it was in the vulgate, or omits some words from a desire for brevity. Hence it comes that a wrong reading is frequently cited in the Zürich edition (Baiter and Halm) as from N: while the formula 'om. N.' must be regarded with special caution. In my *Anecdoton* I have shown that Stephanus was chary of taking over all the Dutch Professor's results, and that Zumpt regarded some of them with justifiable suspicion (p. xlviii, note). I might have added that Nannius was also 'suspect' to C. A. Jordan, who is responsible for the text and apparatus criticus of the *Verrines* in the Zürich edition. Take the passage at ii, ii § 96, *Cedo mihi etiam istum senatorem, ut hoc amplissimum nomen senatorum non modo ad invidiam, etc.* Here Jordan's note runs: '[senatorem] senatorium N (!)', and in supplementing it Halm says that in what follows he writes *senatorum*, against the vulgate *senatorum*, on the strength of Lg. 42, which he quotes as giving the passage in mutilated form, *cedo mihi istum senatorium non modo ad etc.* The intervening words *senatorem ut hoc amplissimum nomen* were in fact omitted, per parablepsiam, in Lg. 42: and Halm adds 'itemque sic in N. fuisse probabile est.' The note of Nannius makes it certain that this particular parablepsia is not to be laid to the account of the copyist of Lg. 42: it had already occurred in the Cluni codex, which was the *Nannianus*, and from which Lg. 42 was directly copied. Again at ii, iii § 180, where by the way Müller once more accepts an inversion—*mihi da inquam* for *inquam da mihi*—on the sole authority of Lg. 42, Jordan includes Nannius among the authorities for the correct reading *qui tibi*, against the vulgate *qui vel*, though with the note, rendered necessary by Nannius's careless method of citation 'in quo (sc. Nanniano) vix credibile est *qui tibi vel* fuisse, quod vulgo post Nannium legitur.'

Nannius was, in fact, a slipshod reporter; and though it was his good fortune to have

access to the Cluni codex, he greatly confused later criticism by the use he made of his opportunity. Here are some examples of his method—or rather his want of method. At Book iii. § 151 he gives in his lemma the vulgate *cupioque te illud defendere*, and supplies from his 'vetus codex' the new reading—accepted by all editors on his authority down to Müller—*cupioque te ita illud defendere*. Lg. 42 gives *cupioque te ita defendere*, and if this was the reading of Cluni we must suppose that Nannius had jotted down *ita* as to be inserted in place of the vulgate *illud*, and then had carelessly left both *ita* and *illud*. In iii. § 184 his lemma is the vulgate *ut HS uno nomine tredecies auferret*. He wants to alter *tredecies* to *terdecies*, and cites from his 'vetus' for brevity, *ut HS terdecies auferret*. This has given rise to Jordan's note 'uno nomine om. N.' (Lg. 42 here has *terdecies*, a variation which, if *terdecies* stood in C, may easily be attributed to the copyist). At iii. § 65 the vulgar text of Nannius's day ran *ut quocum inire convivium nemo unquam nisi turpis impurusque voluisse*. Nannius found in C *quicum* for *quocum*, and reports accordingly 'vetus: *ut quicum inire*' Here he mistakes *vivere* for *inire*, as had been already done by the copyist who invented *convivium* to go with the imaginary *quicum inire*: he lost, in fact, the opportunity of reporting the true reading, *ut quicum vivere nemo*. That this must have stood in Cluni is proved, not only by Nannius's imperfect citation of *quicum*, but also by the fact that it is the reading of Lg. 42 and φ. Here is a good example from Book ii. § 77: *Iustum de vobis iudicem si vultis habeatis*] *vetus, Iustum etiam de vobis iudicem habeatis*. In the note Nannius gives excellent reasons for reading *etiam*. But unfortunately he ought to have inserted *etiam* in place of, and not in addition to, *istum*: *etiam* without *istum* is cited from the *Fabricianus* (= Cluni) and occurs also in Lg. 42 p.m. Jordan's note 'si vultis om. N' may now be deleted: Nannius meant no harm by his regard for brevity, and it was only *etiam* that he cared about in the passage just quoted. In precisely the same way, while inserting *et sclera* at ii. § 39, he carelessly leaves *commonstrem* standing, instead of *demonstrem*: whence Jordan's note *commonstrem* N. On the same principle, N ought to have been credited with *enumerate* (for *numerale*) at iii. § 79, where I cite the following, as a short example, 'Non audio enumerate HS xv] *vetus, HS xii.*' The correct figure is reproduced in Lg. 42, but no one has thought of tying poor Nannius

down to *enumerate*, which he carelessly left standing in his lemma. At iii. § 209 we have another good example of careless citation. The vulgate gave *poteris proferre*, and Nannius wanted to correct *proferre* to *profieres*, which is in Lg. 42, and must have stood in Cluni: but he omitted to eliminate *proferre*, and is consequently quoted 'poteris proferre, profieres' N. As a last example of the way in which Nannius managed to get his notes mixed up, I refer to iii. § 44, without quoting, owing to the length of the passage: any one who examines what Nannius has written there will see that he is not really stating that he found both *populi R.* and *propter hoc* in his 'vetus,' as Jordan understands.

I conclude this lengthy reference to the *Nannianus* by a short enumeration, following the paging of Müller's edition, of passages where the true reading, long ascribed to Nannius alone, has been found to recur in Lg. 42,—i.e. the direct copy, according to my thesis, of the original which Nannius used, and which was no other than the Cluni codex now at Holkham: p. 212, 16 *commissam esse* N Lg. 42 for *commissare* vulg.: p. 230, 24 *et aequitatem* (omitted in the vulgate): p. 247, 6 *iste* (again an omission): 263, 30 where a short lacuna (*condemnarunt horum iudicio*) is supplied by φ as well as N, and recurs in Lg. 42: p. 305, 19 *lucri* N 42 (an omission in the vulgate): p. 306, 8 DCCCL for DCCC: p. 307, 21 *qui* for *quicunque*: p. 308, 29 *iniqua* for *indigna*: p. 312, 27 *uti* for *ut*: p. 313, 21 *quid, si CCC* (an omission in the vulgate): *ibid.* 24 *capiam* for *faciam*: p. 325, 28 *teneo inquam*: p. 343, 24 *per triennium*: p. 354, 15 *non ex eo quod oportuerit*—all omissions in the vulgate: p. 358, 15 *annona* for *anno*: p. 359, 28 *quinos* and 35 *denis non licere*,—omissions, the latter of which was supplied also from the *Fabricianus* (i.e. Cluni), and is found in the *Vaticanus* as well.

The exact agreement between the *Nannianus* and its copy Lg. 42 should be noted also in their propriae lacunae: e.g. p. 337, 37 *allatarum libris Romae in litterarum*, an omission due to *paralepsia* on the part of the copyist of the Cluni codex, and which recurs also in Lg. 45: 291, 24 where the figures *HSI000* do not occur in either: and 215, 24 where both omit *sed ad communem litium aestimationem venisse*. Cases of the agreement between the two cod. in errors may also be cited: e.g. p. 253, 3 *postulant sed* for *postulantur*: 307, 3 *in aratores* for *aratores*. At 319, 20 both give

afficta for *adfecta*, the reading adopted by Müller.

This completes my detailed proof of the fact that the 'liber Nannii' is the same manuscript as the Cluni codex, now at Holkham. Though the identity of this codex with both the *Metellianus* and the *Fabricianus* is already, I venture to think, sufficiently established in my *Anecdoton*, I subjoin a few cogent passages which I have noted in course of preparing this paper: p. 214, 24 *atque obsecraret om.* M and Lg. 42: p. 218, 4 *ex aere facta om.* M Lg. 42: p. 216, 32 *iste mutet domum* M Lg. 42 for *isti domum nuntiatur*: p. 278, 23 *sed M Lg. 42 for sed etiam* of the vulgate: p. 246, 26 *qui consequitur mensem* a lacuna in the vulgate supplied from φ, and now also from Lg. 42: p. 316, 37 *nummum φ Lg. 42, om. rell.*: p. 268, 18 *an negotiator N φ M Lg. 42, om. rell.*: p. 307, 9 *Pro decuma om.* F Lg. 42. Lastly, the form *mensum* is cited from both F and M at p. 266, 11 and as C is extant here, we are able to verify the citation: C too gives *mensum*. Immediately below the codex fails us: but as *mensum* is again cited from N at p. 266, 29 and from M at p. 268, 14 we may infer that if the codex now at Holkham were complete we should find the same reading in these places also.

The recovery of the Cluni codex puts the text of the Second and Third Books of the *Verrines* on a much more stable footing than previously. For the Fourth and Fifth Books it is well known that *Regius 7774* in the *Bibliothèque Nationale* is of full and well-nigh final authority. There remain only the earlier books, and for these I hope shortly to publish the results of my collation of a codex in the British Museum, which ought long ago to have been placed above both the *Wolfenbüttel* MSS. (G¹ and G²) on which, along with the *Leidensis*, editors have so long relied for the proper constitution of the text. Meanwhile I hope I may be considered to have made good my thesis that, for Books ii and iii, H_o. (or rather C) = N F M + Lg. 42.

W. PETERSON.

MCGILL UNIVERSITY, MONTREAL.
August, 1902.

P.S.—Since the above was written, I have been so fortunate as to identify, in the *Bibliothèque Nationale* at Paris, what is undoubtedly the archetype of all the extant MSS. of the earlier part of the *Verrines*. The family of which G² and the *Leidensis* have hitherto been the most conspicuous

members can always be recognised from the fact that the text ends with the letters *singu* (for *singulari*) at ii. 1, § 111. The Paris MS., numbered 7775, which is signed 'Claudii Puteani,' is a thirteenth century codex, with 43 lines to the page; and it is remarkable that no one should have hitherto noticed that its last folio before the lacuna ends with the letters *singu*. The inference is obvious. It is almost inconceivable that the writer was copying from a codex already ending in that imperfect word, and that by

a mere accident he happened to complete his page with it. Unfortunately only four pages now remain of this portion of 7775: the rest gives the tradition of Books IV. and V. already known to us from the Regius 7774, a codex of the ninth century. But enough remains to enable us to classify all extant MSS. of the *Divinatio* and the earlier part of the *Verrines* in the order in which they most nearly reproduce what must now be considered the archetype.

W. P.

REMARKS ON JUVENAL

I.

THE BODLEIAN FRAGMENTS.

THERE is a pretty general agreement among scholars that the lines of Juvenal in Satire vi. recently discovered by Mr. Winstedt are genuine. Two of them 32, 33, are quoted by the scholiast on vi. 348. The terse vigour of the language, the general stylistic peculiarities, the horrible insistence and ruthless unreserve with which the disgusting subject is handled, are in Juvenal's most perfect manner; indeed it is hard to believe that any save he penned those terrible lines. Proof in detail of their authenticity has been given by Professor Ellis, in his able lecture (The new Fragments of Juvenal, p. 9) and by Mr. H. L. Wilson (*American Journal of Philology*, xxii. p. 271 ff.). If any aesthetic argument were wanted, the words *cum quibus Albanum Surrentinumque uoles Iudei somnia uendunt* if any save he penned those terrible lines. Proof in detail of their authenticity has been given by Professor Ellis, in his able lecture (The new Fragments of Juvenal, p. 9) and by Mr. H. L. Wilson (*American Journal of Philology*, xxii. p. 271 ff.). If any aesthetic argument were wanted, the words *cum quibus Albanum Surrentinumque uoles Iudei somnia uendunt*

hunc mimum! would be sufficient.

To me the only internal difficulty is the quantity of *promittit* in line 2

obscenum et tremula promittit omnia dextra.

It recalls uncomfortably similar lengthenings so frequent in the verse of declining Latinity: some examples are given by Bücheler, *Rhein. Mus.* LIV, p. 486: I may add *quo melior uir est longe subtilior illo*, one of the admittedly spurious lines prefixed to Hor. Sat. I. 10; and *aethere se mittit audiique uocatus Apollo* (Baehrens, P.L.M. iv. 209), *stridula cardinibus claudit antica retortis* (Baehrens, P.L.M. iv. 250), *stat similis auro citri mirabilis arbos* (Baehrens, l.c. 311): all however in arsis. This suggests that the

lines may have been composed about the fourth century after Christ, possibly by one of those admirers of whom Ammianus speaks (xxviii. 4. 14): such is the opinion of Bücheler. But it is more natural to consider that there is a corruption in the text: though I am not satisfied with any emendation hitherto proposed. The most ingenious is Mr. Housman's *tremula promittit et*, but Juvenal nowhere else places *et* third word in a clause. Dr. Postgate's *crimina i. q. stupra*, though attractive paleographically, is to my ear rather too recondite. Professor Ellis's *sonnia* sounds to me too modern: it cannot bear the meaning, 'dreams of happiness,' which he gives it, comparing 547 *qualiacumque uoles Iudei sonnia uendunt*, for there *sonnia* is literal; the Jews dealt in the interpretation of dreams from the time of Joseph downwards. I formerly suggested *ibi omnia* (C.R. xiii. 267); which I now retract, and propose *promittit mollia sc. effeminate practices*, cp. 23 *quanto uox mollior*. If *bona* means good things, and *mala* bad things, I don't see why *mollia* should not mean effeminate things, since *mollis* means effeminate; cp. ii. 165, *cunctis ephebis mollior*, ix. 38, *mollis auarus Petron.* 126 *oculorum mollis petulantia*. The man is *mollis similisque cinaedis*. The text being now set right, I think there is no argument from internal evidence that can be brought against the lines, the meaning of which has been brilliantly expounded by Mr. Housman.

Assuming the lines to be genuine we are confronted with an external difficulty. How has it come about that they are preserved in the Oxford MS., Canonici Lat. 41, which I call O, alone, a MS. which agrees to a large extent with the MSS. of the inferior

class rather than with the Pithoeanus? P von Winterfeld (*Berl. phil. Woch.* xix. 793; *Gött. Gel. Anz.*, 1899, p. 895), laying stress on the fact that there are twenty-nine lines on a page of the Pithoeanus, which is the case, and of the Aarau fragmentary MS., has suggested, with the assent of Mr. Housman (*C.R.* xv. 265), that the loss of the lines in most of our MSS. was due to the loss of a page in the archetype. But what archetype? The archetype of P or ω ? P, he says. And yet O mainly agrees with ω . This does not help us much. Now it is important to notice that though, as Mr. Winstedt's collation shows, O disagrees considerably with P, it yet frequently agrees with it in a surprising way, more so than Mr. Winstedt's silence would often lead the reader to suppose. An examination of the MS. has made this clear to me. Here are some instances; i. 68 *fecerit PO fecerat P² ω* , 86 *nostrī farrago libelli est PO est farrago libelli ω* , 114 *habitat PO habitas ω* , 145 *et PO it P² ω* ii. 116 *abrumperē PO abscindere, abscidere ω* , 140 *moriuntur PO morientur ω* , iii. 210 *aerumnae est PO aerumnae ω* , 259 *e PO de ω* , v. 24 *quo PO quod ω* , 63, 64 transposed in PO, 63 *rogatus PO uocatus ω* , 88 *dabitur PO datur ω* , vi. 159, *nudo PO mero ω* , 474 *est pretium curae PO est operaē pretium ω* , vii. 100 *nullo quippe modo PO namque oblitā modi ω* , viii. 163, *dicit PO dicet P² ω* , ix. 26 *quodque taces PO quod taceo ω* , x. 197 om. *ille PO*, xiv. 38 *damnandis huius enim PO damnis huius etenim P² ω* . Also it presents unique or almost unique readings, e.g. i. 169 *anime ante tubas* (so Valla), and, what is more noticeable, readings of this sort which are either probably or certainly genuine, such as ii. 45 *nam plura for hi plura*, iii. 218 *hic Asiano-rum for haec Asianorum or fecasianorum*, iv. 148 *ex for et P*, the word is omitted by ω , vi. 561 *longe for longa P or longo ω* , vii. 130 *Tongili for Tongilli*, xv. 75 *praestant instantibus Ombis for praestantibus omnibus instans*, 145 *pariendisque for capiendisque*.

It follows from all this that (1) O cannot be derived from the archetype of either P or ω , since in the case of all those MSS. the new lines are absent, and (2) that it is an independent witness, as is shown by the strange deviations of its readings. It is an eclectic authority. I am thus drawn to the conclusion that it must represent a recension other than and earlier than the recension from which the archetype of all our existing MSS. is derived; which has been proved to be the recension of Nicaeus (*C.R.* xi. 402). The newly discovered lines I conclude were

excised from the text by Nicaeus for some unknown reason. It follows that the Oxford MS. represents the vulgate text such as it was before the recension of Nicaeus was made. Now the fact that all our MSS. end abruptly at xvi. 60, proves clearly that they all descend from a truncated original in which one or more leaves were lost at the end. This loss, if my hypothesis is true, must have taken place prior to the recension of Nicaeus and that of the Oxford MS.: it probably took place very early during the period of neglect which the poet's works appear to have experienced for some time after his death (Friedländer, i. p. 80 foll.). This may well have been the case; and just as the recension of Persius by Sabinus is preserved in only two MSS., so the Oxford MS. may be the only specimen of Juvenal's text unrevised by Nicaeus.

II.

ON VI. 614 *foll.*

In this connexion must be considered the lines which occur in some, though not in most MSS., either after vi. 601 or after 614, to which latter place they evidently belong: *tamen hoc tolerabile si non | semper aquam portes rimosa ad dolia, semper | istud onus subeas ipsis manantibus urnis, | quo rabidus nostro Phalarim de rege dedisti*. These lines are omitted by editors, and are unintelligible as they stand. As the two recent attempts to explain them do not convince me (Ellis, New fragments of Juvenal, p. 19; Housman, *C.R.* xv. 265), I will venture a third. But as to their genuineness, it is clear that our established text has lost lines in some cases. Such accidents frequently befall texts: thus the Pithoeanus omits lines of undoubted authenticity, e.g. v. 91, vi. 126. 558—559, x. 67. It may well be then that the lines in question are genuine; whether they vanished through accident or through the energy of a Nicaeus cannot be affirmed. It is clear from Valla's note that their omission was of early date (*hi tres versiculi in multis non sunt codicibus quos in antiquissimo legimus codice et Probus etiam refert*). And they are manifestly ancient: they do not smack of interpolation. They have the crisp vigour of Juvenal; they have his allusiveness to a marked degree. The first two lines refer to the Danaids; the third to Phalaris, a potentate affected by Roman poets, and elsewhere alluded to by Juvenal (viii. 81). But the third line does not cohere well with the other two; and it

seems to me that the two former lines should follow 614, where they were found by Valla, the meaning being that the administration of philtres to her husband by the wife would be endurable if it did not finally produce mania, as for example the hallucination that he is engaged like a Danaid in filling sieves with water. The third line I think should follow 617. It clearly refers to the mad emperor Caligula, who turned from a Roman monarch into a perfect Phalaris, and behaved for three years as such. That the line is corrupt is indicated by the variations in the MSS., *quod for quo, rabidum for rabidus, rostro for nostro, Phalari for Phalarim, ede for de rege*. I therefore propose to read *quo* for *quo*, and reconstruct the whole passage thus

tamen hoc tolerabile si non
semper aquam portes rimosa ad dolia, semper
istud onus subeas ipsis manantibus urnis,
et furere incipias ut auunculus ille Neronis,
cui totam tremuli frontem Caesonia pulli
infudit, quae non faciet quod principis
uxor,
quom rabidum nostro Phalarim de rege
dedisti?

i.e. 'who will not do what you the emperor's wife did, when you made a mad despot of our Roman monarch?' On *dedisti=fecisti* I need only refer to Munro on Lucret. iv. 41. I may note that *cum* with perf. indic. is not unusual in Juvenal: see iii. 122. 195. vi. 421, 458, 542. vii. 83, 86.

S. G. OWEN.

A LAST WORD ON THE PROHIBITIVE IN TERENCE.

In his article in the April number of the *Review*, Professor Clement completely loses sight of the only important point at issue between us. I beg just enough space to call it again into prominence.

Every one must admit that, if it can be shown that *ne feceris* and *cave feceris* are emotional and *ne facias* and *cave facias* (generally speaking) commonplace, this fact is well worth noting and forms indeed a very important distinction, quite regardless of what may or may not, be true of the numerous other sorts of expression that have (in spite of my protests) been dragged into this controversy. Any one who will go through Professor Clement's article on Prohibitives in Terence, and exclude all other expressions, leaving merely the instances of these particular prohibitions still classified in every instance exactly as Clement himself classifies them, will find that Professor Clement presents substantially the same results that I myself presented in my original paper. According to his own classification, not one of the instances of *ne feceris* or *cave feceris*, is distinctly a commonplace prohibition, while on the other hand, eleven out of the eighteen instances of *ne facias* and *cave facias* are distinctly commonplace in character—0 per cent. of the perfect tense, 60+ per cent. of the present tense. I showed in the March number of the *Review* that Professor Clement was clearly guilty of several misinterpretations and that the figures should be 0 per cent. of the perfect and 80 percent,

of the present. Judging from his complete silence on the subject in his reply, he accepts my corrections. Whether he does or not, it will be noticed that my claim is completely and explicitly vindicated by its most bitter opponent. The only reason why he does not realize this fact is that he loses himself in a mass of extraneous matter.

The allusions I claimed to have made to unpublished collections in my possession, which Professor Clement has been unable to find after reading my Latin Prohibitive 'more than twenty times' are as follows: p. 137, ll. 34-35; p. 140, ll. 6-7; p. 147, ll. 31-32; p. 148, ll. 21-22; p. 149, ll. 23-24 (cf. p. 148 ll. 27-29). Professor Clement aims to give the impression (*Am. Journ. Phil.* xxii. p. 94, table) that I said there are no instances of *cave* in Catullus, Horace, Vergil, Tibullus, Propertius, or Ovid. As a matter of fact I made no statement regarding any one of these authors that could by any amount of violence be distorted into such an insinuation. Most of the passages which Professor Clement accuses me of having ignored were among my collections and were intentionally omitted for the reason that they threw no light on the particular question I was discussing. And the few genuine cases of careless omission fortunately do not affect the validity of my conclusions.

As Professor Clement makes some serious charges against me in addition to those concerning the poets above mentioned, I must, in justice to myself, call attention also to

the real character of those charges. I take for this purpose, as a fair sample, the most serious charge of all, namely the one involved in the following sentence near the end of his article: 'When his (*i.e.* Elmer's) list of perfects, which he believes to be "practically complete," lacks at least 11 examples of *ne* or *cave* with the second person (Bennett and I have supplied that number), his figures on any subject are likely to be regarded with grave suspicion.' Of these 11 (12 ¹) examples to which he refers, two (Plaut. *Vid.* 83 and 91) were not found, at the time when I was making my collections for The Latin Prohibitive, even in those complete editions of Plautus that contained the fragments of the *Vidularia* (Studemund's discussions and Winter's *Fragmenta* I had not seen, nor have I yet seen Weise's last edition of the fragments). Another, *viz.* Plaut. *Cist.* 300 had not, as yet (with only one exception, if I mistake not), been embodied in the text of the editions of that play. In another, Plaut. *Pers.* 572, the reading regularly found in editions existing at the time was *ne*, *si parseris*, etc., (not *ne parseris*, as since read by Goetz-Schoell and Leo). In another, Plaut. *Truc.* 606 *ne responsis* (Goetz-Schoell) is not, even yet,

the usual, or the accepted reading, nor has it any manuscript authority. In another, Plaut. *Cas.* 404, *cave obiexis* is a mere conjecture in a corrupt line (the MSS. have neither *cave* nor *obiexis*). Another, Plaut. *Truc.* 943 (933 in Ussing) was actually given in my list (p. 142), though the printer's omission of 'Truc.' before 933 seems to make the line-number refer to the preceding 'Pers.' Thus disappear 7 of the alleged careless omissions.

Professor Clement has then convicted me simply of having overlooked 5 (4 ¹) instances (only one prior to Cicero) in my reading of the more than ten thousand pages of Latin from the earliest times down to the end of the Augustan Age—one omission to two thousand pages or more. He has discovered these omissions by re-reading the authors 'in most cases, from two to eight times,' as he tells us. I have no desire to belittle the importance of this achievement. I merely venture to repeat a sentiment expressed in my previous article, *viz.* that a critic who makes such serious charges as Professor Clement has made against a fellow-investigator should be far more sure of his ground.

H. C. ELMER.

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SOME POMPEIIAN MUSICAL INSTRUMENTS AND THE MODES OF ARISTIDES QUINTILIANUS.

In the Naples Museum there is a small instrument, found at Pompeii in 1876, and described in the official catalogue as 'Instrument de musique à neuf tuyaux. Il ressemble à un orgue.' Little interest seems to have been taken in this instrument, which is passed over by Gevaert (*La Musique de l'Antiquité*, Vol. II. p. 301, *Note*) with the single remark, 'Je n'ai remarqué au musée de Naples, parmi les nombreux débris d'instruments pompeiens, qu'un seul fragment de *syringe*'.

Of late years a second instrument has been found at Pompeii, similar in every respect to the above, but larger, and having two additional pipes. For convenience I will refer to these instruments as No. I. (the earlier), and No. II. (the later and larger instrument).

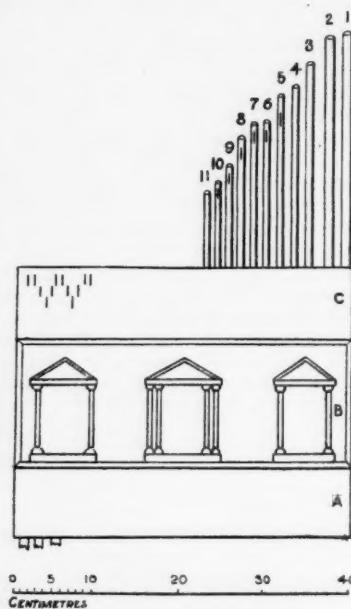
No. II., of which a diagram accompanies this article, being more decayed than No. I., has been laid out on a plaster frame in

exactly the condition in which it was found, and is considered of sufficient importance to be placed in a case by itself. The Museum Authorities were kind enough to take its measurements for me, and to give me permission to photograph it; but owing to its fragmentary condition it was found impossible to place it in a suitable position for photography without risk of serious injury. I believe it to be some kind of portable pneumatic organ, the mechanism of which, being of leather and wood, has entirely disappeared, leaving only the bronze pipes and outer casing.

The three portions or plates, A, B, C, are not joined, but were found in the position shown in the diagram. Their ends are bent back at a right angle to the depth of 14 millimetres. There is nothing to support the pipes, which, if the instrument were placed upright, would simply fall down behind the plates. I believe them to have

been flute and not reed pipes, and that their mouthpieces were of wood.¹

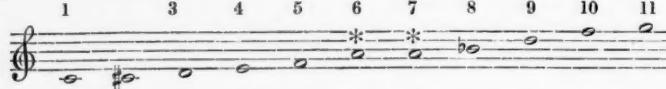
With No. I. were found a few short pieces of bronze which exactly fit the oblong holes in its pipes. These holes, which are at varying distances from the top, are found in pipes Nos. 3 to 8 of the smaller instrument, and Nos. 5 to 10 of the larger.



No. I. has, in addition to its three plates, an oblong piece of bronze containing eighteen holes similar to, and arranged in the same order as, those shown in Plate C of the diagram. In No. II., I counted twelve of these holes, but owing to its corroded condition I could not see if there were more.

At the left hand, projecting from the

Ex. 1.



which I obtain as follows:

Nos. 5 and 10, being in the ratio of 2-1 give an Octave.

Nos. 5 and 1, ratio 20-27 give a 'High'

¹ In 1892 I made a thorough examination of No. I. and found nothing of the nature of metal tongues such as would be used for pipes of this calibre if they were 'reeds.'

lower corner of Plate A, are some very corroded fragments of oblong pipes: similar ones were found with No. I., but detached from it. They appear to have had some connection with the conveyance of the wind, the arrangements for which may have occupied a considerable space below and at the back.

The diagram is to scale. The pipes of No. I. are on the left hand side, the largest being nearest the outer edge of the plate: they are therefore exactly in the reverse position of those of No. II. This, however, may be an accidental arrangement, due to a mistake in replacing the instrument in its resting-place after removal for measurement, and No. II., having been preserved exactly as it was found, is the more authoritative on this point.

I place the measurements alongside of one another for facility of comparison. No. I. has nine pipes, No. II. eleven.

The measurements are in centimetres.

| | No. II. | No. I. |
|--------------------|---------------|--------------|
| Plate A.—40 x 8. | 37 x 7. | |
| B.—40 x 15. | 35.5 x 9.8. | |
| C.—40 x 8. | 37 x 6.7. | |
| Diameter of Pipes. | { 1.2. | 1.45. |
| Lengths of Pipes. | { | |
| | No. 1: 27. | No. 1: 24.5. |
| | ,, 2: 26.5. | 2: 22. |
| | ,, 3: 24. | 3: 20.9. |
| | ,, 4: 21. | 4: 18.8. |
| | ,, 5: 20. | 5: 17. |
| | ,, 6: 17 (1). | 6: 15. |
| | ,, 7: 17 (1). | 7: 13.2. |
| | ,, 8: 15. | 8: 11.1. |
| | ,, 9: 12. | 9: 9. |
| | ,, 10: 10. | |
| | ,, 11: 9. | |

Taking Nos. 5 and 10 as representing the two F's of the treble stave, the scale resulting from the measurements of the pipes of No. II. gives these intervals:

or 'Sharp' Fourth, slightly larger than the perfect Fourth, 4-3.

Nos. 5 and 2, ratio 20-26.5=53-40, between the true Major Third, 5-4 and the Pythagorean Major Third 81-64. Therefore very nearly the major third of equal temperament.

Nos. 5 and 3 ratio 5-6. This interval,

the Minor Third, is given by Eratosthenes and Didymus for the chromatic tetrachord, and by Ptolemy for the soft chromatic. (*Claudius Ptolemy*, Edited by Wallis, p. 171).

Nos. 5 and 4 ratio 20-21. This semitone is given by Ptolemy (p. 177) in the mixed soft diatonic genus.

Nos. 5 and 6, ratio 20-17. No recognised interval.

Nos. 5 and 7. No. 7 gave the same measurement as no. 6. My impression is that a difference formerly existed, that the pipes have become altered by corrosion, and that we have here to do with an enharmonic tetrachord.

Nos. 5 and 8, ratio 4-3. Perfect Fourth.

Nos. 5 and 9, ratio 5-3. Major Sixth.

Nos. 10 and 11, ratio 10-9. Minor tone. This interval occurs in Ptolemy's mixed soft diatonic and mixed intense diatonic genera.

The measurements of No. II. were taken for me in April last by an expert: that the smaller instrument gives no satisfactory results (there is no octave for example), is probably due to my own faulty measurements taken ten years ago.

I have had an oblong hole cut near the top of a conical brass whistle 29.5 cent. in length, by 14 millimetres in diameter: on inserting a piece of brass (of size and shape similar to the short oblong pieces found with No. I.) which fits the hole and divides the passage at this point, I find that the effect is to cause the second harmonic, i.e.

Ex. 2.



'and the diapason exceeds this mode by a tone' he says.

This accounts for the upper six of the eleven pipes of No. II in a remarkable manner, and seems to point to the instrument being the *πτερόν*, the 'Wing' of 'Anonymous.'

A comparison of the series of 11 notes

Ex. 3.



The lists of intervals given by Aristides on p. 21, seem to agree with the practice

¹ *Anonymi Scriptio de Musica*, published with notes by F. Bellermann, Berlin, 1841.

the interval of a 12th from the fundamental note, to sound out clearly to the disadvantage of the fundamental, and first harmonic. The insertion of the bronze tongues in pipes 5 to 10 would therefore probably cause this portion of the scale to be transposed a 12th higher, without increase of wind pressure.

The series of notes given in Ex. 1 forms no regular scale or mode as understood by us; but my impression is that the ancients never used a complete diatonic scale in actual performance. This seems to be shown by the flutes found at Pompeii and elsewhere, and by the series of notes used in the Delphic Hymns, of which I gave an analysis in the *Classical Review* of November 1895.

Bellermann's 'Anonymous,'¹ in describing the various kinds of instruments in use in his time, says ἐμπνευστὰ δε αὐλοί τε καὶ ὑδραύλεις καὶ πτερά. Not one word more is said in the treatise concerning the πτερόν, though the tropes for Lyres, Kitharas, Hydraulis, and Auloi are given. The only other author who mentions the πτερόν is the Hagiopolite (Vincent, *Notices des Manuscrits de la Bibliothèque du Roi*, Paris, 1847, p. 266), who says that the Iastian trope is suitable to this instrument: and here we have an important clue, for it will be found that if pipes Nos. 6 and 7 originally differed in length so as to give an enharmonic diesis, we get the intervals of the Iastian mode given by Aristides Quintilianus (Meibom), p. 21, which he makes to consist of

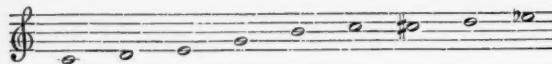
in Ex. 1. with those found on other instruments strongly suggests that ancient melodies and modes did not usually contain all the degrees of any particular octave, but that notes were omitted, in a way analogous to the ancient Scotch pentatonic scale

of musicians in this respect. A few examples will perhaps make my meaning clear.

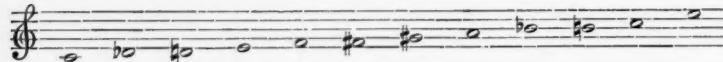
On three Pompeian Auloi, discovered in

1867 we find that the last melody played, closed,¹ occupied these intervals (which I have transposed to facilitate comparison):

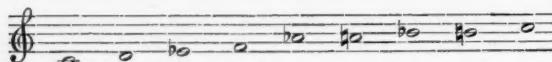
Ex. 4.



Ex. 5.

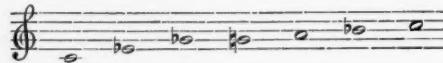


Ex. 6.



On two auloi in the British Museum, found in a tomb near Athens, the series of notes are:

Ex. 7.



Ex. 8.



It will be seen that in all the examples 4 to 8 the diatonic order of tones and semitones is broken by leaps of major or minor thirds, analogous to the leaps which occur in Ex. 1, and which are accounted for by the Iastian mode given by Aristides Quintilianus. None of these examples give what we understand by a Mode, that is a definite series of tones and semitones in the octave; and in this connection it may be well to compare them with the Modes of Aristides.

The Aristidean Modes (p. 21) besides the Iastian (Ex. 2.) are:—

Lydian; composed of Diesis, Ditone, Tone, Diesis, Diesis, Ditone, Tone. This forms an octave.

Dorian; Tone, Diesis, Diesis, Ditone, Tone, Diesis, Diesis, Ditone. This exceeds the octave by a tone.

Phrygian; Tone, Diesis, Diesis, Ditone, Tone, Diesis, Diesis, Tone. This is less than the octave by a tone. (Compare Ex. 1.)

Mixolydian; Diesis, Diesis, Tone, Tone, Diesis, Diesis, Tritone. This completes the octave. The tritone is of frequent occurrence in the melody of the first Delphic Hymn.

Lydian Syntomon; Diesis, Diesis, Ditone, Trihemitone, and Ditone.

These strange modes are explained by a passage in Cleonides or Pseudo-Euclid (Euclid, p. 15, Meibom) in which it is shown that the Lydian; Parhypate Hypaton to Trite Diezeugmenon, is contained between

mesopycna. Its first interval must therefore be a diesis, a quartertone in the enharmonic, a third of a tone, or a semitone in the Chromatic genus.

Dorian; Hypate Meson to Nete Diezeugmenon, is contained between barypycna, therefore its two first intervals are Dieses. But Aristides says that its first interval is a tone, and that it exceeds the octave by this interval. In other words the ancient musicians added a note below its theoretical compass, when using the Dorian mode.²

Phrygian; Lichanou Hypaton to Paranete Diezeugmenon, contained between Oxypyca. Its first interval should be a tone or a trihemitone or a ditone, according to the genus. Aristides makes it begin with two dieses, and says that it is less than an octave by a tone. In practice therefore musicians cut off its lowest note, and made it begin where we should expect the Dorian to begin. The Iastian Octave-species being the same as the Dorian, but a semitone higher in pitch, is contained between barypycna; Cleonides does not mention this

¹ The Auloi were each capable of playing a complete series of semitones (except that one single semitone was absent from one of the instruments). By means of mechanism any holes could be opened or closed at will before and after, but not during performance.

² 'Le ré inférieur est un son supplémentaire, qui ne fait pas partie intégrante de la gamme, et ne sert sans doute que pour certains écarts exceptionnels de la Mélodie.'—M. L. Laloy, in reference to the Aristidean Modes in *Congrès International d'Histoire de la Musique*; Combarieu: Paris, 1900.

mode. Its uppermost note was omitted in practice, since Aristides says that it was 'less than an octave by a tone,' see Ex. 2.

Mixolydian; Hypate Hypaton to Paramese; contained between barypycna, there-

fore its two lowest intervals are Dieses, in accordance with Aristides.

Lydian Syntonon. This is not mentioned by Cleonides. In its Aristidean form it gives us a major and minor triad.

Ex. 9.



In my article in the *Classical Review* of Nov. 1895 I endeavoured to explain my view that the 'system' was the basis of Greek melody rather than the 'mode'; and my examination of these Pompeian instruments tends to confirm me in this view. Aristides says (p. 13), 'some systems are continuous, the sounds being placed in consecutive order; others are irregular, in which the melody does not proceed by consecutive sounds.' The system is merely a series of scale sounds of any number greater than two (Aristoxenus)¹; thus we have the tetrachord system, the pentachord system, the octave system, the greater and lesser perfect systems: but those used for melody, as opposed to these theoretical systems may be 'continuous or irregular.' They and the nomes appear to have been formulas, analogous to the *rāgas* of South India, on which melodies were composed.

It seems evident that the enharmonic and chromatic genera, with their colours, had not died out at the time of the destruction of Pompeii: and that musicians rarely if ever made use of a complete diatonic series of notes for any given melody. It is true

that such a series is found in the three Hymns of the Antonines and in the Tralles Hymn, which are all in the diatonic genus: but none of the Pompeian instruments hitherto discovered seem to have been used for this genus, and though the auloi contained mechanism which would allow of its use, they were arranged in the order shown in the examples, when found.

The varieties of Mode, Colour, Genus, to say nothing of the addition or subtraction of notes from the octave species, must have placed immense resources in the hands of musicians unaccustomed to harmony. We are wont to look upon their instruments as meagre and of small musical value. This is true of them from a modern harmonic standpoint; but their meagre compass must have been to a large extent compensated for by the resources which the very absence of harmony placed at the command of the performers.²

C. F. ABDY WILLIAMS.

² Since this article was written I have been kindly given an opportunity of examining a hydraulic organ, constructed by the Rev. F. W. Galpin. Its scale of 19 sounds gives the six modes mentioned by Anonymous as those used on this instrument; and on running the fingers rapidly over the keys at random I was charmed by the attractive character given to the music by the modal arrangement of the notes.

THE SEXAGESIMAL SYSTEM AND THE CRADLE OF THE ARYANS.

IN his notable paper entitled *Die Urheimath der Indogermanen und das Europäische Zahlsystem*, Berlin, 1890, Johannes Schmidt took it for granted that the Indo-Europeans originally had a pure decimal system, and argued that the many important traces it possesses of a sexagesimal system are an intrusion from the numerical system of the

Sumerians of Babylonia, and prove that the European branch of the Indo-European race early dwelt near enough to the Euphrates valley to come under the influence of the Sumerians, therefore, in Asia, and that thus all Indo-Europeans originated in Asia. Schmidt summed the matter up as follows (p. 52): 'Wo diese gegend zu suchen sei,

wissen wir nicht. Die einwirkung braucht keine unmittelbare gewesen zu sein, da der handel die sexagesimal rechnung auch durch zwischenliegende länder anderer zunge hindurch getragen haben kann. Jedesfalls aber ist der schauplatz derselben so lange in Asien zu vermuten, bis unwiderlegliche und zwingende beweise für Europa beigebracht sein werden.' This is certainly shifting the burden of proof without warrant. Schmidt agrees (p. 46), that the use of 60 in Chinese is also derived from Babylon; but the distance from Babylon to the extreme west end of China is greater than that from Babylon to Italy. Hence, even if we should grant that the numerical system of the Europeans was affected by that of the Babylonians, we are by no means forced to admit that they lived in Asia at the time this happened. And this must be the final judgment on that portion of Schmidt's paper that deals with the origin of the Indo-Europeans, cf. Hirt, *I.F.* i., p. 468, Kretschmer, *Einleitung*, p. 58.

So far as my knowledge goes, Schmidt's is the only recent attempt to re-establish Asia as the cradle of the Indo-Europeans, and he himself insists (p. 54), that all previous attempts to prove the Asiatic origin of the race were failures. On the other hand, the general trend of such evidence as has been brought forward of late goes to make it very likely that the original seats of the Indo-Europeans were in northern Europe, or somewhere along a line extending from central Europe to middle western Asia (Penka, *Die Herkunft der Arier*, Kretschmer, *Einleitung*, p. 60, Brugmann, *Grundriss*², i., p. 22). Indeed, one theory (Sweet, *History of Language*) has gone farther and made it appear at least plausible, not only that the barbarians of the stone age of Europe were the ancestors of the Indo-Europeans, but that their language was adopted from a conquering population of Asiatic invaders. The modern representatives of these are the Finns, and their ancient cousins were the Sumerians. Now, the Zyrian Finns, as well as the Europeans, have a break in their numerical system between 60 and 70 and, like the Europeans, form the tens from 70 on in a way different from that employed in the earlier tens (Schmidt, p. 41, Grimm, *Geschichte*, p. 256). The advocates of the Finnish origin of Indo-European speech, far from being disturbed by Schmidt's argument, can welcome the facts he brings forward and see in them another argument in favour of their position. For, if the primitive inhabitants of Europe

were so thoroughly under the influence of their Asiatic conquerors that they gave up their native tongue and adopted that of their masters, it would be most natural that they should also adopt their numerical system. That the numerical system of the Sumerians was developed after they settled in the valley of the Euphrates and was not common to them and their kin when they still dwelt in their more northerly homes, is something that we certainly have no right to take for granted. Moreover, it is not at all necessary for us to assume (Schmidt, p. 48), that the mixture of the decimal and the sexagesimal systems which we find in the European languages, is due to the grafting of an Asiatic sexagesimal system upon a native Indo-European decimal system. For the system of the Sumerians, as well as that of the Zyrian Finns, is itself a mixture of decimal and sexagesimal (Cantor, *Geschichte*, p. 93, etc., Schmidt, p. 47).

Ignoring all theories as to the original numerical system of the Indo-Europeans, we must recognise the historical fact that when we first meet them they are in possession of a mixture of the decimal and the sexagesimal systems, and that, the farther back we probe, the more we find of the sexagesimal element. At an early day the decimal element got the upper hand, and that, the farther south, the sooner. We know that the Germanic languages have the most numerous and distinct traces of the decimal-sexagesimal system, the shock being much used in Germany, and the long hundred lingering in England, and being the usual hundred in Iceland. In a paper entitled *Greek and Runic Letters and Numbers*, to appear in *The American Journal of Philology*, I have shown that in early Teutonic, there were at first no tens above the shock, or 60, and that those that the Teutons later possessed, developed from the long hundred down. The Slavs have the *kopa*, corresponding to the shock of the Teutons. The Celts, Romans, and Greeks had a break in their numerical systems at 60 that betrays an earlier condition similar to that of the Teutonic system. Professor Minton Warren calls my attention to the fact that the Romans also retained distinct traces of the long hundred, of which the most interesting is the notion that 120 was the span of human life.¹

¹ Tacitus, *Dialogus*, c. 17: *centum et viginti anni . . . unus hominis aetas*. Censorinus' quotation (de d. n. 17) from Varro's *Antiquitates*, book xviii: *ut traderent historici de Romuli urbis condendae auguriis ac duodecim vultuviis, quoniam*

In the paper referred to above, I showed that the Greeks originally made a stop at 600, though we find them early supplied with the remaining hundreds. The break made by the Indians after 50 (Schmidt, pp. 42, 48), is probably a modification of an older break after 60; just as the Slavic system (as doubtless correctly assumed by Schmidt, pp. 41, 52) is a modern levelling of an old sexagesimal system. Still, the only certain trace of the sexagesimal system among the Indians is the fact that, like the Babylonians and Chinese, they divided the day into sixty parts (Cantor, pp. 91, 92), and used 60 and 600 as indefinite large numbers, as we have found the Teutons, Latins, etc. using 60 and 600. Had the Indo-Europeans received the sexagesimal element of their numerical system from the Sumerians, it would be reasonable to expect that we should find this element best preserved among those of their descendants who remained nearest this old and powerful centre of civilisation. The facts are, however, just the reverse. That is, the Teutons, those Indo-Europeans that lived farthest from Babylon, preserve best the decimal-sexagesimal system. And it is just these Teutons who are, and have always been, so far as we know, the nearest neighbours of the Finns, themselves the possessors of a decimal-sexagesimal system.

But, whether they evolved it or adopted it, and whether they adopted it from the Finns or they and the Finns received it alike from distant Babylon, the decimal-sexagesimal system is the earliest numerical system that we have any right to ascribe to the Indo-Europeans. How it arose, and how it gradually yielded to a purer decimal system, I should explain as follows. The most primitive means of counting is based on the fingers, or the fingers and toes. Hence the decimal and the vigesimal systems, both common among most races of the world in reckoning small and moderately large numbers. Before the affairs of ordinary life demanded the frequent use of very large numbers, the observation that the year had about 360 days and the division of the circle of 360 degrees into six equal

arcs, cut off by the application of the radius, introduced the sexagesimal system (Cantor, p. 92). This was a sort of learned system, and at first was doubtless not used or needed in the affairs of ordinary life. It naturally blended with the decimal system already in use for small numbers, and, when the affairs of ordinary life were so far developed as to require the use of larger numbers, this being the only available means of expressing them, was naturally resorted to, clumsy and inconsistent as it was.

That the large unit, 60, was broken up into 12×5 , and that the use of 12 is thus based on the earlier use of 60, seems improbable to Hirt (*I.F.* i, pp. 468, etc.). It is really very simple and natural. In keeping count of things that are being measured—for example, bushels of grain that are to be stored away, or gallons of wine that are being drawn from a cask—the people of primitive times doubtless chalked them off just as farmers and merchants do to-day. That is, they made parallel strokes until they had four, and then they crossed them with a fifth , symbolizing the four fingers and the thumb of one hand. It took twelve such groups to make a shock. It might be asked why they did not make nine strokes, and then cross that. For the simple reason that  can be counted at a glance and  is hard to count. Similarly, small objects, like coins, were counted by being touched and slid aside one by one, first twelve with the little finger, then twelve with the ring finger, etc. Certainly Hirt has not succeeded in disproving Schmidt's contention that 60 arose independently of 12, and that we have to deal with a decimal-sexagesimal system, and not with a decimal-duo-decimal system.

That in this decimal-sexagesimal system the decimal element in time got the upper hand, was due to the gradual development of the need and the use of large numbers in business, and in those mathematical calculations that had nothing to do with astronomy or the reckoning of time. In this way, that system that had always been the usual one in small numbers extended upward, and thus crowded out the astronomical system that had been imposed upon it. That is, 10×10 took the place of 10×12 , though it long permitted the long hundred and the shock to persist in limited use, especially in certain conservative trades in which any large unit would suffice; and so, too, 10×100 (or 1000) displaced 10×120 (or 1200) but tolerated 600 some time before it made it yield to 500. This tallies exactly with what we

CXX annos incoluisse praeferisset, populum Romanum ad MCC perverturn. Here we have 12, 120, and 1200. For the whole matter see the discussion by Gudeman, *P. Cornelii Taciti Dialogus de Oratoribus*, Boston, 1894, p. 186-187. Also Pauly-Wissowa under *centuria*, p. 1961: *Centuria est quadrata in omnes quattuor partes, ut habeat latera longe pedes MMCD, Varro de r. r. i., 10, 2; also, da 1 iugurum = 2 Quadratactus zu je 120 römische Fuss im Quadrat, etc.*

observe of the relative advance in civilisation among the Indo-European peoples. Those that first had need of large numbers, early passed the 60-line, and even the 600-line, while the people of the North, who were longest in emerging out of the condition of primitive life, were also latest in

crowding out the astronomical elements that made inconsistent the essentially decimal character of their numerical system.

GEORGE HEMPL.

ANN ARBOR,
April, 1902.

NOTES.

DERDAS THE LITTLE.—Aristotle, *Pol.* viii. (v.) 10, 10=1311 b 3, *καὶ ἡ* [scil. ἐπίθεσις] *Ἀμύντου τοῦ μικροῦ ὅτῳ Δέρδα διὰ τὸ καυχήσασθαι εἰς τὴν ἡλικίαν αὐτοῦ.* Nothing is known elsewhere of Ἀμύντας ὁ μικρός; and the reference in *εἰς τὴν ἡλικίαν* is entirely obscure. The old Latin translation, which regularly translates ἡλικία by *etas*, here (and presently at line 18) turns ἡλικία by *statura*. I suggest that the words *τοῦ μικροῦ* are out of place, and that we should read *ἡ Ἀμύντου ὅτῳ Δέρδα τοῦ μικροῦ*. Derdas was short of stature, and Amyntas offended him by a taunt on the subject. If asked to account for the transposition, I should further suggest that the words *τοῦ μικροῦ* were originally a *gloss*, which has found its way into the text at the wrong place. We may accept the usual view that the Amyntas mentioned here is the father of Philip, and that Derdas is the brother of Eurydike, Amyntas' wife (Xen. Hell. V. ii. 33, foll. iii. 1 foll.; Theopompus ap. Ath. x. 436 C). There is no ground for holding, with Droysen, *Geschichte des Hellenismus*, I. 78, that Derdas killed Amyntas. Aristotle distinctly says that he is dealing with conspiracies both successful and unsuccessful; *οἱ μὲν ἀρρεθησαν, οἱ δὲ ἐπεβολεῖθησαν* (1311 b 35). Schäfer is no doubt right in holding that it was one and the same Amyntas who, with some intermissions, occupied the Macedonian throne from B.C. 394 to B.C. 370.

E. S. THOMPSON.

* * *

ON CULEX, 93, 94.—‘O gratissima Tempe fontis Hamadryadum.’ For the corrupt ‘fontis,’ Mr.

Housman (dissatisfied with Leo’s ‘hortus’) has (at p. 340 of the current volume of the *C.R.*) proposed ‘frigus’ (i.e., ‘sedes frigida’). I would suggest, (as being nearer to the tradition and giving an appropriate sense) ‘saltus’—*s* (*f*)=*f*, *a*=*o*, *ll*=*n*, *u*=*ti*. Compare Virg. *G.*, iii. 40, ‘Dryadum siluas saltus que sequamur.’

SAMUEL ALLEN.

* *

AN EMENDATION OF TERENTIANUS MAURUS.

578, 579. Lachm.

a brevis dum consonanti t propinquae iungitur tempori suo fab eadem consona (-nae M) partem trahet

Lachmann altered *ab* to *de*, which can hardly be right, nor can much be said for *ex* of Keil, still less for the violent alteration of Werth de Terentiani sermone et aetate (Teubner 1896), p. 336.

Temporis vocalis eadem consonae partem trahet.

Surely Terentianus wrote *als*, as Tertullian, or whoever was the writer, quoted by Hand Turs. I. p. 7, although as a rule *abs* precedes consonants only (Sandy on Cic. Orat. § 158), adv. Marc. iv. 4. 103 *Quos in euangelio dominus quoque dixit et haedos Abs oribus cerni stantes in parte sinistra;* v. 5. 122, *Promissa abs alio.*

ROBINSON ELLIS.

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REVIEWS.

CLARK'S ORATIONS OF CICERO.

M. Tulli Ciceronis Orationes. Vol. vi. : Pro Milone, Pro Marcello, Pro Ligario, Pro Rege Deiotaro, Philippicae I—XIV. Recognovit brevique adnotatio critica instruxit ALBERTUS CURTIS CLARK. Oxonii. E typographo Clarendoniano.

FOR obvious reasons Mr. Clark has begun his edition of the speeches of Cicero in the *Scriptorum Classicorum Bibliotheca Oxoniensis* with the sixth and final volume. In editing

the Miloniana he is on ground which he has made his own : but even here he has by no means been content with reprinting the apparatus of his edition of 1895. Of course the Turin palimpsest, and the *quadriga* of German MSS. retain their pre-eminence. But as the better representatives of the class of *deteriores* he now takes a Berne MS. *b*, the *cod. Barberinus* *β*, and an Oxford MS. *σ* (which is not the same as Halm's *Salisburyensis*), of which only the last was

regularly used for the *editio maior*; so that δ, the symbol for the commonest readings of the *deteriores*, does not mean quite the same as before. The text naturally remains for the most part unaltered. In § 36 Mr. Clark now deletes *civium*, which he was previously content to bracket, a safer course, seeing that it is given by all MSS. In § 42 he rightly adheres to his neat conjecture. In § 60 his new conjecture *hic* seems preferable to the alternative suggestion. In § 67 he well prints *non*, which is much more likely to have been corrupted than *quod*, easier as that correction is palaeographically. In § 69 *proxinorum* does not call for more than the modest doubt which is now expressed. In § 75 *P. Aponio* seems certain. In § 90 *cui mortuo unus* is rightly retained. I am inclined to think that *quae oblio* in § 99 can be defended, a view which I am glad to find Dr. Reid sharing. It might have been better to make it quite clear what is the reading of *H* in § 105, i.e. *comprobavit*, so that the text given rests on a correction. I agree with Dr. Reid that it is hard to believe in *maneret* in § 77. But on the whole one could not desire a sounder text.

In the *Caesarianae*, avoiding alike the niggardliness of Baiter, and the needless lavishness of Halm, Mr. Clark has used 12 MSS., 7 of which he has himself collated. These fall into three clearly marked classes, of which the best consists of an Harleian (2682, second copy), a Vossian, and an Ambrosian. The second class is marked by many interpolations; the third keeps closer to the first, but has also many interpolations. Perhaps Mr. Clark has gone a little too far in rejecting as interpolations words omitted in one MS. only: e.g. in pro Deiot. § 34 *liberi* may well be sound, and only one Harleian omits it. At all events it would be safer to bracket than to strike out. On these speeches Mr. Clark has not many suggestions of his own to offer, but *tum* for *cum*: in Deiot. 29 is good, and in § 35 *aliquid quid* is admirably neat.

For the *Philippicae* the Vatican MS. retains the primacy which it has enjoyed since Halm, although Mr. Clark justly abstains from assigning too much weight to the omissions of the first hand. The rest have come from a mutilated archetype, and none of them show such marked superiority that others can be safely neglected. Some of these Mr. Clark has collated afresh, notably a Vossian MS. at Leyden. The elaborate paper which Mr. Clark published in this *Review* (xiv. 402-411) saves a reviewer the

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pains of selecting the noteworthy new readings of this text; but a few may be picked out for comment. In i. 35 the correction *carus* for *clarus* seems certain: and is excellently shown to turn the scale in favour of *iucundus* as the correction of *unctus*. Excellent too is Mr. Clark's reading in ii. 8: in ii. 55 *proficerit* is good palaeographically and makes good sense. In ii. 76 he is fully justified in printing *an ut tu*, and in ii. 91 the defence of the reading of D is cogent. In iii. 26 the correction *Gellius* is very happy and can hardly be doubted. In v. 6 Mr. Clark's emendation is the simplest and most satisfactory as yet suggested; in § 11 it is less convincing. § 12 can hardly be regarded as satisfactorily healed as yet. Mr. Clark's method of dealing with vi. 10 seems to me admirable, and *quasi* is good in viii. 20. The transposition in ix. 16 is an easy and attractive one. In x. 5 the correction of *nam* to *ne* is very neat, and in x. 19 the emendation *capessendum* is irresistible. In xi. 38 the reading *cuquam* is to be accepted with both hands. In xii. 24 the change of *oderint* into *occiderint* can hardly be doubted. In xiii. 19 the clause *eque—aut* seems rightly bracketed. In § 25 *deminutus* is almost certain. Dr. Reid's brilliant *dirutus aere* rightly finds a place in the text in § 26, and § 27 is well cured. In § 33 *ri expulsum* for *in expulsum* is excellent. In § 34 the corruption is not very simply removed; in § 35 Mr. Clark's correction is greatly to be preferred to Madvig's; and in § 37 *quam* seems certainly right. Mr. Clark has done well to get rid of *Caesarem* in § 41. In § 42 *sponte adivi* is clever, but finds its right place in a note rather than in the text and perhaps similar caution might have been used in § 47. In xiv. 13 Dr. Reid's bold excision seems to be the best way of dealing with the passage.

Various possibilities are open to the editors of the different volumes of the Oxford *Scriptorum Classicorum Bibliotheca*. Some from the nature of the case have had to be content with sifting materials already accessible, and giving a well-weighed revision of the current text. Mr. Clark has been more fortunate in both respects. He has made important additions to our critical material, and has also proposed not a few emendations, which deserve to find general acceptance. His text of these orations is not only abreast of the best critical science, but in many points it marks a distinct advance.

A. S. WILKINS.

E E

PHILLIMORE'S *PROPERTIUS*.

Sexti Properti Carmina. Recognovit breviique adnotatione critica instruxit JOANNES S. PHILLIMORE (Scriptorum Classicorum Bibliotheca Oxoniensis. Clarendon Press. 1901).

To this text of Propertius, the work of an editor who on more than one side was well qualified to render service to his author, we turned with high expectations. Unhappily these have by no means been fulfilled. It may at once be said that to all intents and purposes Prof. Phillimore leaves the text of Propertius in the state in which it was twenty years ago. We do not in particular expect to find in an editor subsequent to Bährrens such an exaggerated opinion of the merits of N as is enunciated on p. 3 of the preface to this edition: *unum N ceteris universis pluris faciendum*; and it is especially hard to see how any one can hold such a view after studying the brilliant series of articles by Prof. Housman in the *Journal of Philology*, vols. xxi., xxii.: particularly pp. 149 *sqq.* of vol. xxi.

Let us take a few examples of inferior readings in this edition due to N. At ii. 20, 7-8, we derive from N a most improbable construction of *defluere*:

*nec tantum Niobe bis sex ad busta superbe
sollicito lacrimas defluit a Sipylo.*

At iii. 1, 23 O reads:

omnia post obitum fingit maiora vetustas.

N has *famae* post obitum—*vetustae*, which Prof. Phillimore adopts, with the violent change of *vetustae* to *vetustas*. The difficulty of *famae* in this reading is great: *maiora* again is felt to be insufficient without some such word as *omnia*: in fact the reading of O is decidedly superior. Nor again is N to be followed in the unintelligible reading of ii. 30, 19:

*non tamen immerito! Phrygias nunc ire per
undas egs.*

for *nunc* (*num*) *tu dura paras Phrygias*, etc., the scribe having taken *non tamen immerito* from iii. 19, 27.

Such a text as that before us can only be explained as arising from an undue conservatism, which has replaced the *lues emendatoria* of the early part of last century. It may be that, in the presence of influences hostile to study of the Classics, the students

of Greek and Latin texts are less inclined now than before to emphasize the undoubted corruptions which exist therein. In Germany there has arisen of late a school of editors whose leading principle seems to be to construe 'through a stone wall.' Among the advocates of this principle is Rothstein, who is singled out for special praise by Prof. Phillimore, as *in tradita codicium auctoritate vindicanda felicissimus* (*Praefatio*, p. v.). That is the verdict of our critical editor on one who in editing Propertius declares that 'überhaupt kritische Zwecke dieser Ausgabe zunächst fern liegen,' and one is almost forced to conclude that such aims were equally remote from the plan of the present edition. Yet to attempt to edit Propertius without 'kritische Zwecke' is to begin with an initial misconception of the duties of an editor in the presence of an avowedly difficult author and an avowedly corrupt tradition.

But although our editor is such a staunch upholder of 'the MS. tradition,' there are times when even his suspicions are aroused: he has exercised his powers of emendation in several passages of Propertius, and, if we may believe a reviewer in the *Athenaeum* for Aug. 2 last, his success in emending is as great as his skill in defending the text.

Let us examine the corrections by which this reputation is said to be earned.

(1) I, 7, 15-18:

*te quoque si certo puer hic concusserit arcu,
(quod nolim: nostros te violasse deos!)
longe castra tibi, longe miser agmina septem
flebis in aeterno surda iacere siti.*

On v. 16 Prof. Phillimore remarks *nondum sanatus (versus)*. His own contribution to the healing process is the change in the punctuation of the line which once ran (*quod nolim nostros te violasse deos*). In the old form the line was a natural parenthesis, instinct with the soft melancholy which broods over Propertius's best work: with the altered punctuation it becomes jerky and abrupt. 'If Cupid's arrow were to smite you (I should be sorry: think of your having offended mine own deity!),' then etc. The parenthesis is now different from anything that we meet elsewhere in Propertius: see for example the soothing leisureliness of i. 7, 4 (*sint modo fata tuis mollia curminibus*) or i. 20, 2 (*id tibi ne vacuo defluat ex animo*). Moreover, taken

closely with *nolim* the perf. infin. *violasse* is quite in accordance with the idiom by which the perf. infin. accompanies verbs of wishing (Gildersleeve and Lodge, 280, 2 (b)); with Prof. Phillimore's reading we must translate 'think of your having offended—': but Ponticus has not yet provoked Cupid (cf. the futures at vv. 18 *sqq.*).

Ov. Fasti iv. 122, *a!* *nolim victas hoc meminisse deas*, quoted by Rothstein, is modelled in point of cadence on the line in Propertius, and is another argument for not breaking the verse into two parts.

(2) II. 12, 17-18:

Quid tibi iucundum est siccis habitare medullis?
Si puer est, animo traice puella tuo.

Here at v. 18 Prof. Phillimore conjectures *animo* (*aīo*) for *alio*: he also retains *puer*, which is given up as the second word of the verse by all modern editors except Bährrens and Müller. As Mr. Housman pointed out (*Journal of Philology*, vol. xxii. p. 112) *si pudor est* is employed in mild or playful remonstrance: cf. Prop. i. 9, 33 *quare, si pudor est, quam primum errata fatere.* Even Rothstein is unable to support *puer est* or *puella*, which is in all probability a correction based on *puer*, in some measure because of the seeming metrical irregularity of *traice* as trisyllable. For other cases where mistaken ideas as to scansion have corrupted the text cf. ii. 34, 40, iv. 8, 58.

But what is the meaning of the line as Prof. Phillimore gives it? Apparently it is 'if there is a youth to be found' (not one who, like the poet, is world-weary and disillusioned) 'transfix him, O Maiden, with your passion.'

It is surely hard to believe in the Latinity of *animo traice—tuo*. And verses 17 *sqq.* are addressed not to Cynthia but to Cupid. *isto—veneno* for example v. 19 is the poison of Cupid's arrows: cf. ii. 13, 1-2. The use of *puellae* in v. 23 instead of *tua* is also in favour of this view.

II. 32, 5:

cur autem Herculeum deportant esseda Tibur.

So Prof. Phillimore for the vulgate *cur vatem—.* N has *cur uate.*

Apparently *autem* means 'moreover' in this passage. But that is not a possible meaning of the word. We should expect *etiam.*

II. 34, 83.

ne minor his animi est: aut, si minor ore,
canorus
anseris indocto carmine cessit olor.

Animi est is Prof. Phillimore's correction for *animis* NDV *animi* FL. It is to be presumed that the sense is 'nor is he (Virgil) less in point of genius in these (the bucolics); or, if his verse be inferior, then the melodious swan has retired with the tuneless strain of a goose.' There seems to be very little point in the addition of *ore* which ought to have a strong emphasis. It is much better to maintain the contrast between native genius and stateliness of diction given by the reading of Housman

nec minor hic animis, ut sit minor ore,
canorus
anseris egs.

'And Virgil, no less inspired here (in his bucolics), though his diction is less stately, is not like a melodious swan retiring, etc.'

III. 17, 17:

dum modo purpureo tumeant mihi dolia musto.

Prof. Phillimore conjectures *tumeant* for *numen, numerem, nūne*, of the MSS. basing his emendation on Aetna 271 (267) *horrea uti saturent, tumeant et dolea musto.* Palaeographically *tumeant* is not improbable, and, as regards the sense, is an improvement on *sument* of the vulgate.

Postgate, who independently conjectured *tumeant* (v. *Classical Review*, xv. p. 409), has well shown how the various corruptions could arise from his reading *cumulēt*: cf. Ov. Tristia III. 10, 72 *nec cumulant altos servida musta lacus.* ('On certain Manuscripts of Propertius,' p. 41): and this reading I am inclined to prefer.

IV. 4. 55:

Si hospes patria metuar regina sub aula.

patria metuar, is Prof. Phillimore's correction of the MSS. readings *patrianne tua* F *pariamne tua* N *patiare tua* DV. He also reads *sin* for the MSS. *sic.*

Again there is some doubt as to the meaning of the proposed reading. Is *hospes* vocative; or is it nom. to be taken with *regina*? The latter is presumably the case, as there is no comma before or after *hospes*. The sense is then 'but if I shall be feared a stranger queen neath a palace roof in mine own land, the betrayal of Rome is dower enough.' Objections to this are (1) *sin* is

out of place, for *si minus* states the second alternative at v. 57; (2) Tarpeia could not be a *hospes regina* merely because she was the wife of King Tatius who was *hospes*; (3) the idea of being feared is unnecessarily obtruded, and *metuar* is therefore open to much the same accusation as *pariam*.

IV. 11, 53:

*vel cui, iuratos cum Vesta reposceret ignes,
exhibuit vivos carbasus alba focos.*

Cui iuratos for *cuius rassos* of the MSS. is due to Prof. Phillimore. That *cui* is necessary has been seen long ago. But *iuratos* is a long way from the indications in the MSS. involving as it does a change in three letters. *vel cui, sacra suos*, the reading of Bährrens, is distinctly nearer to the MSS.: *sacra* being abbreviated *sra* we should have first *vel cui sra suos* and then by the transposition of one letter *vel cuius rassos*.

I have thus gone through the list of emendations which are spoken of in the *Athenaeum* as being 'very happy.' It will be seen that I am not disposed to go so far, and still less do I admire the emendation of IV. 5, 19 *sq.* which Prof. Phillimore suggests in the footnote, but with timely prudence does not place in the text.

Though the text of Prof. Phillimore's edition is, as has been said, marked as a rule by excessive deference to the authority of N, once or twice we find him deserting the indications of that manuscript without due cause, as at ii. 27, 7, where he adopts the reading of O *flemus* (*fletus* N *fletis* G). The correction of Prof. Housman, *fles tu*, is decidedly preferable from the point of view of symmetry (cf. *tuis*, v. 10) and also nearer to the *ductus litterarum*. Again, at iv. 1, 31 *Luceresque Soloni* is the reading of N: other MSS. have *coloni*, which Prof. Phillimore adopts. But, not to speak of the improbability of *Soloni* being an interpolation, it has been proved to be the true reading by A. Dietrich, *Rhein. Mus.* 55 (1900), p. 201. As early as 1873 R. Ellis defended *Soloni* in a review of Paley's *Propertius* (*Academy* for February). It may here be observed that Prof. Phillimore has made a fresh collation of N, which, however, he was unable to finish. He has also traced back to Beroaldus readings which had been previously attributed to German or Italian scholars. For these services he deserves our thanks.

There are a few mistaken ascriptions in the critical notes. Thus at ii. 28, 62 the punctuation is wrongly ascribed to Postgate

(v. his critical note). At iii. 11, 5 *ventorum* is due not to S. G. Owen but to Postgate (*Journal of Philology*, ix. p. 68), to whom also is to be ascribed *ossa a* at iv. 5, 64 (*Transactions of Cambridge Philological Society*, vol. i. p. 386).

In p. 1, line 5 of the *Praefatio, Guelferbytanum* is apparently a slip for *Guelerbytum*.

Our view as to the merits or demerits of his edition ultimately depends on how we answer these two questions:

(1) Is it safe to regard any one MS. of an author as an infallible guide?

(2) In the case of Propertius, should N be so regarded?

On both these questions we cannot do better than cite the reviewer already referred to.

'A MS. invested with supreme or rather unique authority is apt to prove a hard master; and without impugning the pre-eminence which is claimed for N, we think that too much respect has been shown [by Prof. Phillimore] to its readings on the whole. Certainly it makes the poet more obscure and incoherent than ever.' This is, so far excellent: but the words that follow, perhaps by a later hand, betray the loose thinking prevalent on this subject. 'At the same time, if the principle is sound, as we believe it is, Prof. Phillimore deserves credit for having carried it out thoroughly.' The italics are ours. It is wrong to trust N: and yet it is right. Probably when Prof. Phillimore trusts N, the results are happier than when less skilled conjurers try their hand. Under his gentle and soothing spells N may become quite docile and friendly. But the ruder treatment of others it resents.

It is plainly necessary for editor and reviewer alike to make up their minds whether an author is to be regarded as corrupt, or nonsensical by nature. But in the case of Propertius a plot has been laid to brand him as corrupt: 'of late years Propertius has been largely re-written by various hands: and the new recension was threatening to supersede the traditional text.' Thanks to the responsive nature of N, alluded to above, this insidious attempt on the part of the various hands to represent their own handiwork as Propertius has been foiled. They may have thought that they were basing the new text on a thorough examination of the MS. evidence: but no: they were re-writing Propertius.

In conclusion, we think that the book is retrograde in tendency; and that it is markedly inferior to the text which it has

supplanted in Oxford Examinations, viz. that of the late Professor Palmer. And as we have been unintentionally assisted before by the *Athenaeum* reviewer of this edition we have to thank him for another sentence in conclusion: 'If dead poets may be supposed to interest themselves in the fate of

their writings, Propertius we fancy would turn over these pages with equal astonishment and gratitude.' With half of this last statement the present writer is quite able to concur.

J. ARBUTHNOT NAIRN.

ENGELMANN ON THE MSS. OF STATIUS' *SILUAE*.

De Stati Siluarum codicibus. Dissertatio inauguralis quam ad summos in philosophia honores ab amplissimo philosophorum ordine Lipsiensi rite impetrando scripsit ARTHURUS ENGELMANN Arnstadiensis. Leipzig. Hirschfeld, 1902. Pp. 144.

THE matter no less than the confident tone of this Leipzig dissertation will raise a commotion in Germany. Dr. Engelmann (for we may assume that he obtained the distinction at which he aimed) seeks to undermine the credit of the recent Teubner text by lowering the Madrid Manuscript of the Siluae of Statius from its pedestal of lone pre-eminence and elevating once more at its side Politian's excerpts from the 'ancient codex of Poggio' as an equal and indeed superior witness to the tradition of the text.

Dr. Engelmann's materials are derived not from an examination and recollection of the copy of the *editio princeps* preserved in the Corsini collection at Rome, into which Politian copied these excerpts, but from photographs of the same belonging to the University of Heidelberg and lent by it for the purposes of this inquiry. Dr. Engelmann asserts that the readings extracted from the 'etus Poggii' may be distinguished throughout from the other marginalia of Politian by the character of the ink with which they were written, a faint diffusive fluid which contrasts strongly with the thicker and more stable of the other annotations. He holds that this difference is enough, and was considered enough by Politian, to identify the readings given as those of the V.C., even if no other definite indication is appended. He further claims that his statements as to what these lections really are should be believed rather than those of his predecessors because the characters are clearer in the photographs than in the original and because he used all diligence in examining them and in cases of doubt referred to the well known expert, Prof. Zange-

meister, for his opinion. The care and method observable in Dr. Engelmann's dissertation undoubtedly go some way to justify his claim. All the same it is to be regretted that he did not append to his book a reproduction on a reduced scale of one of the Heidelberg photographs which would have materially assisted his readers to form a judgment of their own upon a question at once disputable and intricate. It is not yet too late, we may hope, for this to be done. It is the more necessary because Herr Vollmer, who has edited the Siluae, studied the Heidelberg photographs without making Herr Engelmann's discovery.

For his complete collation (pp. 112-140) of all that appears as marginalia in the Corsini copy Dr. Engelmann is entitled to the gratitude of every student of the text, and to these for this, if for no other reason, his dissertation will be indispensable. The value of the conclusions at which he arrives in the course of his disquisitions is, however, another matter entirely. A lengthy examination of these cannot here be undertaken, and as already hinted it will doubtless be forthcoming elsewhere. I may recur to the subject before long in a different connexion. In the meantime let the following observations suffice.

That the Matritensis (M 31), a full collation of which was published in this *Journal*, vol. xiii. (a fact of which Dr. Engelmann betrays no knowledge), is not the 'etus codex Poggii,' should be settled once and for all by the fact that according to the express testimony of Politian, the 'etus' did not contain the line I. 4. 86a, and the Matritensis does contain it. If it is not, then, for all the good the Corsini copy is for the textual criticism of the Siluae, it might be flung into the Tiber. This is the view of the Teubner editor Herr A. Klotz who, confident that he knows better in 1900 what the 'etus codex Poggii' was than Politian did in 1500, declares it to be the Matri-

tensis and Politian's manuscript a copy of this, and accordingly banishes the excerpts from his critical apparatus (praef. p. lxxiii.). Herr Engelmann retorts by declaring that the Matritensis is itself a copy of the 'Vetus Poggii.' With this in view he attempts to write the history of the text of the Madrid MS. by means of an examination of its corruptions. In his solution of this stratigraphical problem as we may call it, which belongs to a class, whose importance in Latin textual criticism is, as I have pointed out more than once, still very imperfectly appreciated, he is not what we should call successful. He spends 16 pages in arguing from internal indications that the exemplar of the Matritensis was a fifteenth century MS. and the exemplar of this a ninth century one. One of his arguments is that the scribe confuses *c* and *t*, letters not formed alike till the thirteenth century; hence the exemplar of *M* was not older than the thirteenth; nor again was it written between the thirteenth and the fifteenth century, because 'Iam duobus fere saeculis (all italics are mine) antequam Poggii Florentinus Constantia prefectus monasteria propinquae codices ueterum scriptorum inuestigandi causa uisit (i.e. in 1416) in monasteria quae inde a nono usque ad medium fere tertium decimum saeculum litteris tam egregie floruerunt ut monachorum industriae permulta scriptorum antiquorum opera memoriae tradita debeamus, tanta luxuria et pigritia inuaserant ut monachi libris describendis non iam uacarent et uel artis scribendi plerumque imperiti essent,' while in the sequel some particular cases of this ignorance are cited, the earliest of which is 1291. Look first at the arithmetic. Dr. Engelmann requires a full 200 years for his argument; and the utmost he can get out of these data is some 150 years (1416 to 1266); so it has to jump half a century or so. Then consider the ratiocination itself. It assumes that the conditions of writing were the same all over the western world unless indeed Dr. Engelmann can show that the original which Poggio had copied was always in the place where he found it. One other point only I will refer to. If there is one thing certain about the exemplar of the Matritensis, it is that in its script *r* was of a form which the fifteenth century scribe readily mistook for *s*. The proof of this is that in the *praefatio* to the first book he wrote *s* for *r* wholesale, but soon learned better; after that the corruption is only sporadic. Now Dr. Engelmann uses this confusion to show that the manuscript from which the exemplar was copied

belonged to the ninth century! But though the results of his investigations are fruitless, he must be commended for having undertaken them. His collections of the MS. corruptions will be very useful to future inquirers. His errors are partly those of inexperience, and he has at least some notion of how to set about this sort of task, more a good deal than his countryman at Marburg who has attempted to estimate the value of the 'Lusatic codex of Propertius'; nor is his failure after all much more conspicuous than that of the skilled critics and palaeographers who have essayed a similar problem, though perhaps of greater intricacy, in the case of the text of Catullus.

Dr. Engelmann, as the reader will have seen, holds a brief against the Matritensis, and therefore cannot be relied on to recognise its merits where he should. Thus he admits that 'in matters of orthography the silence of Politian cannot be trusted' (p. 105), which means that a scholastic of that time did not trouble himself about such minutiae (cf. p. 108, 'neglectis minutis orthographicis nonnullis a Politiano'); yet on p. 96 he argues in favour of the 'humericus,' 'humidus,' 'humentibus' of the excerpts against the uncorrupted spelling of the Matritensis. A more serious matter is his treatment of the crucial passage, in the encomium of Rutilius Gallicus i. 4. 83 *sqq.*, which stands thus in *M*:

Libyci quid mira tributi
obsequia et missum media de pace tri-
umphum
laudem et opes tantas nec qui mandauerat
ausus
expectare fuit gaudet Thrasymennus et
alpes (86a)
attollam cantu gaudet Thrasymennus et
alpes (86b)
Cannensesque animae primusque insigne tri-
butum
ipse palam laeta poscebat Regulus umbra.

Here, in spite of what the Teubner editor has most justly observed upon the subject (praef. p. lxxii.), Dr. Engelmann reverts to the view that 86a is interpolated. But interpolators aim at making a text more intelligible to the average mind: they do not introduce lines which no commentator on Statius even is able to construe. Herr Klotz is clearly right then in contending that the last half of 86a has been lost through a dittography of the last half of 86b. His proposed supplement <*sed quid tua, Gallice, facta*> is, it is true, unsatisfactory; but this is no argument to convict

the scribe of M or any one else of fraud. What has fallen out is of course irrecoverable: but the following restoration gives the general sense

Libyci quid mira tributi
obsequia et missum media de pace tri-
umphum
laudem et opes, quantas nec qui mandauerat
ausus

expectare fuit, <praeclaris impare factis>
attollam cantu? gaudet e.q.s.

Herr Engelmann then cannot claim to have solved the problem which he has attempted: but he has done something to clear the way towards its solution.

J. P. POSTGATE.

BECK'S CUPID AND PSYCHE OF APULEIUS.

L. Apulei Fabula de Psyche et Cupidine.
Praefatus atque interpretatus est J. W.
BECK. Groningae apud J. B. Wolters.
1905. Pp. xxii, 100.

'in der That giebt es kein einziges antikes
Kunstwerk, welches auf die Erzählung des
Apuleius zurückgeführt könnte.'

The recent edition of the Metamorphoses by J. Van der Vliet made it unnecessary to provide the text with a critical apparatus. Dr. Beck has followed Van der Vliet with but few variations: in iv. 31 he reads *effici* with the best MSS., instead of *efifice*, regarding the former as a colloquial form; in vi. 9 he is led by the context to read *errati*, instead of *irati*; and in vi. 11 he accepts Traube's brilliant conjecture *genuacei*, where Van der Vliet reads *aurei cubiculi*. There are also some slight deviations in orthography.

In accordance with the programme stated in the preface, the commentary discusses in a broad general way matters of common linguistic interest, as well as the special characteristics of Apuleius. Some of the longer notes summarize the present state of our knowledge on various questions and are stimulating to further investigation: such are those on *gemens ac fremens*, iv. 30; *domitionem*, iv. 35; *merendo fluctuat*, v. 21; *protinus pergit*, v. 27; *de fluvio*, vi. 12, in connection with which the text of the *Iusurandum Argentoratense* (842 A.D.) is given with a version in classical Latin; *alioquin*, vi. 15. Many remarks are of interest to the student of colloquial Latin and to the Romance philologist.

Errors and misprints are few: on p. 45 instead of iv. 35 we should read iv. 34, and on p. 92 *snavi* should be *suavi*. The suggestion on p. 51, that *se* in Hor. *Epist.* ii. 2. 107 has the force of *inter se*, does not seem to be warranted by the sense of the passage.

On the whole the commentary is suggestive and stimulating, especially to those who have not kept track of the work of the Munich school of Latinists.

J. C. ROLFE.

FOWLER'S HISTORY OF GREEK LITERATURE.

A History of Greek Literature. By HAROLD NORTH FOWLER. N.Y., 1902. Pp. 501. Price \$1. 40.

THIS work, containing some five hundred pages and belonging to the 'Twentieth Century Series,' is intended for secondary schools and colleges, but its use by cultured people in general is kept in view. The difficulties, or rather the impossibilities, imposed by these conditions, are as nearly overcome by the author as seems practicable. Though the author lays no claim to originality, still his treatment is independent so far as the great authors of the classical period are concerned. The work covers the whole range of Greek literature from the beginning to the Byzantine period. It is written in an agreeable style with good perspective and sound judgment. In the general survey, perhaps a clearer conception of what is meant by Greek literature would have been advantageous. Sometimes 'literature' is conceived in its modern sense, and works are dismissed, or even ignored, as not belonging to literature, while others of a like sort receive attention. The line is notoriously hard to draw, and perhaps the

best method would be not to attempt to draw it at all, but to include everything which has come down to us in writing from Hellenic antiquity. This would necessitate a chapter on inscriptions; but such a chapter is really needed in a work of this kind. To cite a single instance of the fault referred to, there seems to be no good reason why an account of Aristotle's logic should be given while Apollonius of Perga, who left us a treatment of Conic Sections by means of coordinate axes not inferior to anything which Descartes produced, is dismissed with the statement that he 'was a famous mathematician and astronomer.' The English illustrations of Greek verses are faulty in several respects. Ambrose Phillips's translation of Sappho's Hymn to Aphrodite does not by any means 'show the rhythm of the original.' No 'general reader' ignorant of Greek would make an Adonic out of 'Even though thou wouldest not,' and nobody can make one out of 'Palace golden.' The Sapphics, too are not Sapphic. But to dwell on all inaccuracies, real or imaginary, would do injustice to a really meritorious work.

M. W. H.

LAMARRE'S HISTORY OF LATIN LITERATURE.

Histoire de la Littérature Latine, depuis la fondation de Rome jusqu'à la fin du gouvernement républicain. Par CLOVIS LAMARRE, docteur ès-lettres, etc. Ouvrage couronné par l'Académie Française. 8vo. Tom. I. Pp. xii, 494; Tom. II, p. 640; Tom. III. p. 598; Tom. IV. p. 472. Paris, Ch. Delegrave. 30 francs.

THIS elaborate history of Latin literature under the Republic well deserves the recognition which it has gained from the French Academy. From the literary point of view it is excellent; and for the general reader, desiring to get full information on the early Roman writers, with a liberal allowance of specimens of their style, nothing better could be desired. But it cannot be said to make any great contributions to our knowledge; or to be of much importance for the student. The scholarship is in some places

of a penultimate or even an antepenultimate type. The author, after a lengthy discussion of the early languages of Italy, comes to the conclusion that we must find in Oscan the fundamental element of Latin, from which it gradually separated itself under the influence of Greek; nothing is quoted on the metre of Plautus which has been published within the last thirty years: on the Ambrosianus of Plautus there is no reference to Studemund's apograph; Publilius Syrus still appears as Publius. On the other hand the work is written from a first-hand study of the writers, whose works are described and analysed with only too great fulness, and illustrated by many and happy quotations from modern, especially French literature. Dr. Lamarre has rather wisely thrown his longer illustrative quotations together, into an appendix which forms Vol. IV, and includes 192 passages from authors

ranging from Ennius to Varro, with a translation *en face*. These translations, like the shorter ones interspersed in the text, are free and spirited but usually accurate, though a point is missed when (I. p. 234) 'vidi ego te astante ope barbarica' is translated 'je t'ai vue dans l'éclat de tes richesses,' and (p. 253) the significant 'hera' is omitted. He has given himself plenty of space. Three hundred and sixty pages are assigned to Cicero, exclusive of a hundred and sixty taken up with illustrative quotations; so that there has been room to give a pretty full account of all his works. One misses both in the biography and in the

treatment especially of the philosophical treatises the critical note. From Dr. Lamarre's easy flowing narrative the reader would gather little notion of the storms that have raged round the man and his work. For the present writer Mommsen is not so much refuted as ignored. In short, to repeat my previous impression, Dr. Lamarre is not so much the scientific historical student of literature, as himself a diligent and agreeable *littérateur*. But what he has seemingly aimed at, he has achieved with a good deal of success.

A. S. WILKINS.

BARTAL'S GLOSSARY OF LOW LATIN IN HUNGARY.

Glossarium mediae et infimae Latinitatis regni Hungariae jussu et auxiliis Academiae Litterarum Hungaricae. Condidit ANTONIUS BARTAL, Socius ascriptus Academiae Litterarum Hungariae. Lipsiae, in Aedibus B. G. Teubneri (Budapestini, sumptibus Societatis Franklini). 1901. 4to.

WE gladly welcome this elaborate and learned work on Mediaeval Latin, published by the Franklin Society and the Hungarian Academy of Literature at Buda-Pesth (and Leipsic, Teubner).

Every word printed, and briefly but adequately explained in Latin and Hungarian, in this substantial volume of xxviii. and 724 quarto pages, costing 50 marks, occurs in one or more of the Hungarian works enumerated on pp. xxi. to xxviii., including Latin writers down to the present day.

A good many of the words are already recorded in Du Cange's and Diefenbach's Glossaries, but these the author has marked 'Du C' or 'Dief.', and sometimes explained better and more fully.

In his preface the author gives many illustrations of the character and formation of Hungarian Mediaeval Latin words, some being derived from classical Latin by means of pre- and suffixes, or alterations in the spelling, while others are vernacular words put into a Latin dress by the same means. The author naturally speaks only of the 'Latinitas Regni Hungariae,' but his explanations are equally applicable to the Latinitas Angliae, Franciae, Germaniae,

Italiae or Hollandiae, etc., as each country formed its Mediaeval Latin in the same way.

In perusing one or two of the books from which the author has made his collection, it strikes us that he might have made his work still more serviceable by reading his sources a little more systematically. For instance, we miss *squassus* of the *Statuta Civit. Curzulæ*, ed. Hanel, p. 60, cap. 132. Du Cange records it from another source, and its meaning is evidently a shaking, a punishment inflicted on a nobleman who had injured another married nobleman. But Du C. has: 'dent ei 5 *squassus* sive botte curli vel turture,' whereas Hanel prints 'debeat habere 3 *squassus* curuli.'

We also miss the form *bullare* (a punishment) of *Stat. Civ. Curz.* p. 7 (cap. vi.), while the author has only the corrupt *bulare* from a later source. He records *pellis mardurina*, but not *pellis marturina* from *Ljub. Mon. Slav. i.*, cap. 3, nor *pellis vulpina* (*ib. i. 2*). We do not find the *solidus romanatus* (*ib. i. 5*), though the author records the simple *romanatus*. He does not give *nauclerius* (*ib. i. 14*), while *havere* quoted from A.D. 1248 (*ib. i. p. 75*) already occurs in A.D. 1187 (*ib. p. 13*). And when the author records *stolium* (a fleet), why not *stolus* (*ib. i. p. 13*)?

If space permitted we could refer to more omissions, and other particulars which we should wish to have found in this Glossarium. In making this remark, however, we do not mean to diminish in any way the great merits and value of M. Bartal's Glossarium, which every library with any regard for scholarship ought to possess.

We conclude this necessarily short notice with observing that, under *arenda* (locatio and conductio), an Hungarian author is quoted who derives this familiar word from the Span. *arrendar* (to let, farm out), or 'better and more probably' from the

Hungarian *áron-adás* (allotting or farming out for money) or from *áron-adá* (pretio dedit). M. Bartal mentions no other etymology of our *rent*.

J. H. H.

CORRESPONDENCE.

SOME RECENT NOTES ON SOPHOCLES' *OEDIPUS TYRANNUS*.

I HAVE no desire to emulate the example of my humorous adversary of the early seventies, but I cannot help remarking that, in Prof. Phillimore's interesting notes on Soph. *Oed. Tyr.*, it remains uncertain whether he is aware of an edition which is not yet out of date.

On 779-781 it needed no Mr. Whitelaw to defend *περπάος*, which Peter Elmsley found attractive, as I mentioned in 1871; nor is *ὁ ταῦπος* otherwise than Greek. But I felt and still feel that the abrupt introduction of such a metaphor is unlike Sophocles.

I have with me here only my first edition of 1871. In this I find that my rendering of 1276-1280 was

'Both pupils rained blood upon his beard, nor ceased from pouring the wet drops of gore, but from both at once was showered the dark red hail.'

The same meaning is given in my translation (1896) except that *όμοι* is 'all at once.' If I took *οὐδὲ* *ἀνίεσσαν* otherwise in the interim, it may have been because I doubted whether *μνδώσας* was not otiose in the passage when so understood.

My first edition also agrees with Prof. Phillimore, if I understand him, on 694-6.

And I am glad that, on 44, 45, where

Prof. Kennedy, following the lead of Musgrave and Thomas Young, adopted an interpretation which I had the courage to ignore (*unde illae lacrimae*, forgotten by all save him for whom they fell) recent editors are substantially agreed. I cannot see, however, that much is gained by emphasizing the *fortuitous* aspect of *ξυμφορά*. When once it is realised that the separation of wisdom from success,—of *εὐβονλία* from *εὐτυχία* (whether due to *τύχη*, *θεός*, *δαίμων*, or *μοῖρα*) was natural and familiar to the Greek of the fifth century B.C., *καὶ* is fully accounted for. 'As a general rule, Fortune' (or 'Heaven')—no matter which) 'favours experienced counsel.'

I still think that in 880 (*πάλαισμα*) any particular reference is out of place. But if such be required, that suggested by Musgrave, who is followed by Linwood, is more immediately appropriate to the action:—'Thebanis *πάλαισμα* est, progressus quem in indagando Laii percussorem fecerant.' The general use of the singular of nouns in *μα* (*μίμημα*, *ἐπίταγμα*, *ἐπιτίθενμα*) is common enough in Plato.

LEWIS CAMPBELL.

Oct. 15, 1902.

DEAN VINCENT AND WARREN HASTINGS.

MR. MOORE SMITH says that 'the day which Dean Vincent honoured by his verses to Warren Hastings was December 18, 1816. But Vincent died on December 21, 1815. The verses must have been sent, if not written, within a week of the author's

death and in 1815, when Hastings entered upon, not when he completed, his eighty-fourth year. Mr. Smith has been misled by the ambiguity of the symbol 'aet'.'

J. SARGEAUNT.

Oct. 14, 1902.

ARCHAEOLOGY

TWO GREEK VASE CATALOGUES.

Catalogue des Vases Peints du Musée National d'Athènes. Par M. COLLIGNON et L. COUVE. (*Bibliothèque des Écoles Françaises*, Fasc. 85.) Paris, 1902.

Catalogues des Vases Peints de la Bibliothèque Nationale. Par A. DE RIDDER. Première Partie: Vases Primitives et Vases à Figures Noires (11 plates and 50 cuts). Paris, 1901.

THE appearance of new Vase-Catalogues is always a satisfaction to the student of Classical Archaeology, and we hope it will not be long before every one of the principal vase-collections has its scientific and well-illustrated catalogue. French scholars, such as M. Pottier and M. de Ridder, are certainly setting an excellent example in this respect.

Of the two volumes before us the former appears under somewhat melancholy circumstances. The original catalogue of the vases in the Athens Museum, published by M. Collignon in 1877, had been rendered sadly out of date by the enormous series of new discoveries in Greece, with which the vase-collection has been enriched since that time, and by the incorporation of the valuable collections of the Archaeological Society (formerly in the Polytechnion) and of the Public Instruction Department. These enlargements led to the consideration of a revised catalogue and the work was undertaken by a young student of the French School, M. Louis Couve, whose promising career was cut short in October 1900 at the age of thirty-one. His labours on the vases, to which he had devoted eight or nine years, were then nearly completed, and to his predecessor, M. Collignon, was entrusted the task of revising the work and seeing it through the press.

The present volume includes all the pottery now collected under the roof of the National Museum with the exception of that in the Mycenaean Room, and the Acropolis fragments, the work of cataloguing which is in other hands. M. Collignon's Catalogue included 821 vases; the present one, in some 660 pages, describes no less than 1988. Moreover, we are promised a supplement which shall contain an index and plates of vase-forms. One is however tempted to ask why the index could not

have been completed in time to combine with the original volume, to say nothing of the plates of shapes which can hardly have been delayed by considerations of time or expense. And this leads us to what at the present time must be regarded as a very serious defect in any catalogue of archaeological objects, the entire absence of illustrations. We do not demand the elaborate reproduction of the most remarkable vases —this in most cases has been done elsewhere—but M. Pottier's *Atlas* of the Louvre vases has shown what excellent results for the student's purpose can now be obtained by photography, even where, as in the lustrous vases, it had previously failed; and it is a mere truism to say that a few illustrations are worth pages of description. The Athens collection is too important, and contains too many unique and rare varieties, to excuse the total omission.

In the classification of the early fabrics—admittedly a difficult matter—there are a few small points in which this catalogue proves disappointing. There is no attempt at a classification of the Cyprus specimens (p. 8, Nos. 34 ff.), and No. 35, an example of the 'Graeco-Phoenician' period takes precedence of No. 36, a 'white-slip' bowl from a Brouze Age tomb! Under No. 37 the reference to Ohnefalsch-Richter's *Kypros* appears to be incorrect; the vase there given is in the British Museum. The Mycenaean pottery (in which only Attica is represented) is curiously lumped with the Cypriote and Island fabrics as 'style primitif'; primitive it may be, but to be unable to differentiate it from the pottery of the preceding age is to revert to the state of knowledge of the 'sixties. We have also been somewhat puzzled to disentangle the class of vases usually known as 'proto-Attic,' which are variously distributed under 'Style Géométrique' and 'Ancien Style Ionien et Corinthien.' Still worse is the fate of the Proto-Corinthian pottery which is jumbled up with Phaleron and Boeotian Geometrical wares; but most astonishing of all is the combining of 'Ionian and Corinthian style' under one heading. If the studies of Pottier, Boehlau, Duemler and other scholars have had any definite result, surely it is the establishment of a distinct Ionic school of vase-painting, developed on entirely different lines from those of Athens and Corinth. M. Couve's published writings

showed him to be so well acquainted with early vase-fabrics and their respective characteristics that we fail to account for this haziness and confusion displayed in his Catalogue.

Apart from these blemishes the descriptions are terse and clear, the technical details adequately indicated, and the bibliographies full and correct; but on the whole the volume is a great disappointment.

M. de Ridder's description of the vases in the National Library at Paris presents several remarkable contrasts to the preceding volume both in contents and appearance. The history of the collection seems to have stopped short about the year 1870; consequently there is an entire absence of many early fabrics which have since become familiar. On the other hand a modern system of classification, so far as it has been possible, has been very carefully observed. In the matter of illustrations M. Pottier's excellent example has been followed, and eleven photographic plates provide admirable reproductions of nearly fifty vases out of 356; these are supplemented by fifty text-illustrations. The chief glory of the collection indeed, the Arkesilas cup (No. 189), does not reappear, but this is hardly a matter for complaint, seeing that it was adequately reproduced in the companion Album, recently published by Milliet and Giraudon. If the volume has any fault, it is its unwieldy size, which, as long as it remains unbound, renders a rapid falling to pieces inevitable.

The present volume extends down to the end of the black-figured style; the second will contain, besides the description of the later vases, the necessary indices and a history of the collection.

H. B. WALTERS.

WISSOWA ON ROMAN RELIGION.

Religion und Kultus der Römer von Dr. GEORG WISSOWA. München, 1902: Beck. Pp. xii. 534.

PROFESSOR WISSOWA has made the study of Roman religion so peculiarly his own that the work, which is now under review, may be regarded as the last word upon the subject for the present. He has gathered the most important materials, and taken account of most of the leading investigations and has treated each succeeding topic with a masterly fulness. He begins with a lucid

sketch of the history of Roman religion. Upon the most primitive stage there break in usages and changes derived from the peoples with whom one after another Rome came in touch, the Etruscans, the Greeks, the eastern nations. This section ends with the worship of the emperors, and the victory of the Christian faith. Next, Prof. Wissowa takes 'the gods of the Roman state religion.' The third and last section of the work deals with 'the forms of divine worship.' In arrangement, therefore, Dr. Wissowa follows to some extent the corresponding volume of Marquardt. And there is also a close likeness of method. The material native to Rome is carefully gathered and sifted, but the author, of set purpose, leaves untouched the comparisons which are furnished by folklore and anthropology, and thus throws down a challenge to the school of Mannhardt and Frazer. 'The mode of treatment of the so-called comparative mythology,' he says, 'has confused the subject rather than helped it' (p. 12). Hence he passes over in silence many of the books which have most influenced English students. Heloses less than we might have expected. The historian who follows the order of time, and keeps close to his authorities, can almost bid defiance to speculation about his facts. And so, whatever ideas we may have about the interpretation of Roman usages, we can avail ourselves with advantage of a work so comprehensive and accurate and cautious.

Yet he has lost something by his refusal to advance beyond the method of Marquardt. The theory of evolution is not so colourless as some would have it, and does not mean simply that each successive stage of development is conditioned by the preceding stage. This much at least was known before Darwin and Spencer, and it is at this point, I venture to say, Professor Wissowa makes a halt. He does not allow for the orderly sequence of changes, by which what is primitive and undeveloped leads up to that which is more developed, and so he makes the mistake sometimes of reading into primitive thought and usage ideas which belong only to a later stage. I will try to show this with reference to several leading topics.

Religion on its intellectual side, advances from ideas which are attached to visible objects—things, animals, human beings and the actions of all these—to ideas which are no longer 'implicate' as the psychologists would say, but spiritual and free. Hence fetishism in which chance objects are regarded as the abodes of divine influences, comes in early times before men apprehend

clearly the abstract idea of a god or gods. The stone was sacred before it was attributed to this or that deity; note the striking phrase *vivum saccum*. Hence it seems that Prof. Wissowa inverts the order of facts when he speaks of 'Terminus' as a surname which first denoted a part of Jupiter's power, and then was separated from him as an individual deity (p. 46). I am afraid that we have here an error which vitiates much of the speculation upon which even Prof. Wissowa ventures at times. He says (p. 318) that 'the presupposition for the whole worship of the gods is the feeling of dependence upon the divine power and providence' (*religio*). Now this is perfectly true for the later stages of Roman religion, but it does not give us the proper starting point for the explanation of what is primitive. The methods of Roman law may properly afford us guidance. 'All law,' says Gaius, 'pertains either to persons or to things or to actions.' And this last term referred originally to visible acts, the touching of a balance, the taking of a clod of earth and so on. Hence the proper name for Roman religion in its own tongue is not *religio*, but *sacra*. We ought to start, therefore, from the notion of *holy objects*, i.e., persons, things, actions; and, by so doing, to determine the primitive idea of holiness. Robertson Smith in the *Religion of the Semites* has laid down the lines upon which such an inquiry is to be conducted. At Rome the notion of magic is closely connected with the notion of holiness, it is the magical act, no less than the sacred person or sacred thing, upon which stress is laid, as for example in the *legis actio*. Unless, then, we start with a correct idea of what is meant by *sacra*, we run the risk of misunderstanding the term *sacrificium*. Hence while it is true that human sacrifice was not practised at Rome in the sense of communion with a deity, it is a serious mistake to deny it altogether (p. 31). Roman religion, in its earlier forms, remains at the magical view of sacrifice, as for example in the *fordicidia* and the sacrifice of the October Horse, and human sacrifices of this type certainly took place. But there are traces of a more developed view still. I have tried to show elsewhere (*Worship of the Romans*, 255 ff.) that the mode of capital punishment can sometimes be traced to human sacrifice, and conversely, the modes of capital punishment may help us to infer the forms of sacrifice. The gallows of to-day is doubtless descended from the tree in which Odin sacrificed himself. Is there anything like this at Rome?

It would seem so. The Vestal Virgins were punished for unchastity by being buried alive. It is instructive to compare this with the *self-sacrifice* of Curtius, which was all the more a sacrifice for being voluntary. Hence it seems unnecessary to isolate the sacrifice of the Gauls (who were buried alive in the forum) as foreign to the spirit of Roman worship (p. 355). If it is dangerous to argue from later ideas in explaining early institutions, it is also dangerous to argue from later institutions. Prof. Wissowa, I venture to suggest, lays too much stress upon Roman religion as a state religion, and imputes to early Rome that centralisation of which the empire was the first effective expression. Just as Jupiter is declared to include the powers which he delegates to Terminus, so, we are told (p. 411), 'the community delegated to a particular clan the exercise of the religious duties which fell within the province of a particular worship.' The contrary seems more probable. It was the clan rather than the community which was the first repository of those clan worships which we can trace at Rome.

Now it is quite possible to reject this or that explanation of particular usages which is furnished by the comparative method, but taken in mass these explanations throw so much light upon the history of religion, that they strengthen each other, and confirm the validity of the method. If it were possible to state the bare fact and to leave it, the student of Roman religion would find in the pages of the *Religion und Kultus der Römer* all that is important. But it is not possible. Prof. Wissowa, as we have seen, cannot keep clear of theories of interpretation, and his theories need considerable qualifications. So much for the general scheme of his work.

When we pass on to a detailed consideration, there can be nothing but praise for the elaborate and careful scholarship which is entirely worthy of Prof. Wissowa's reputation. In a book containing thousands of references there are not more than one or two errors or misprints. At least, if there are, a close reading has failed to disclose them to me.

One of the most interesting parts of the work is that which deals with the spread of foreign influences within the sphere of Roman religion. I am not sure that Dr. Wissowa allows quite enough to the Phoenician and Carthaginian influences which came indirectly by way of Etruria. In Semitic religion Baal and Astarte and

their son are the chief deities worshipped. They had fewer competitors for popular favour and so were more impressive to the foreigner than the gods who severally made up the larger pantheon of Greece or of Rome. Just as the Babylonian supremacy in Palestine left traces in primitive Hebrew religion, so a similar local supremacy of an Etruscan family may very well explain the curious analogies which can be traced at Rome between the worship of Hercules and the Semitic worship of Baal and his son. Caere furnishes the point of contact. But this question has not yet been thoroughly investigated. The dedication of a temple to the goddess of Eryx in 217 B.C. looks like an attempt to entice the Semitic deity over to the side of Rome, and is almost certainly a case of *evocatio*; the invitation of a god 'to leave a previous abode and assume the new one assured to him in Rome' (p. 39).

I know that some people are very sceptical about these suggestions of foreign influences, and especially when we are dealing with the beginnings of Roman history. It is time to ask, however, whether this scepticism has not been carried too far. Recent discoveries have thrown back the beginnings of European international politics to an era which preceded the foundation of Rome by at least a millennium. And the speculations which delighted older schools of historians are being revived in the light of ascertained facts. Perhaps it is impossible to speak with certainty about Semitic religion as a factor in the early history of Roman belief. But when we come to a later date the case is altered. For instance, the influence of Egyptian religion can be studied from both sides. And those who are interested in such matters, may find the reports of Apuleius about the worship of Isis confirmed in nearly every detail by the Egyptian texts. Dr. Wissowa's section, 'Isis und die Götter Aegyptens' (pp. 292-299) may be illustrated by one or two references. Unfortunately Lafaye's *Histoire du culte des divinités d'Alexandrie*, on which Dr. Wissowa appears to have relied, is very inadequate, as the author deals almost exclusively with Greek and Roman sources, and says next to nothing about the abundant Egyptian material; material which has recently been made accessible in the works of Erman and Wiedemann. Dr. Wissowa has been led into mistake when he says (p. 298): 'a *profeta* seems to have stood at the head of the numerous priesthood, who is probably identical with the high-priest mentioned repeatedly by Apuleius; another priest of

high rank was the *grammateus*.' *Profeta* is the translation of the Egyptian term for the ordinary priest *hen-neter* 'servant of God,' and is therefore not the high priest (Wiedemann, *Religion of the Ancient Egyptians*, p. 9). *Grammateus*, again, translates the phrase *kher-heb* reciter-priest. The organisation of the Egyptian priesthood differed in different temples, and there was no settled hierarchy. *Profeta* and *grammateus*, then, are the usual terms. The 'strict selection of animals for sacrifice' (p. 297 n) had not so much a ritual significance as an economic one. Sheep are rarely mentioned in Egyptian texts. Oxen and geese were reared almost exclusively, and so were usually the victims for sacrifice (cf. *Book of the Dead*, Budge's ed. rubric to c. 125). The instructions about washing and abstinence, which are given to the convert in Apuleius's *Metamorphoses*, find a very close parallel in another rubric (*op. cit.* c. 63). The mysteries of Isis and those of Osiris were practically one and the same, and there is little doubt that the Roman proselyte looked beyond the grave to a resurrection and union with Osiris, like the Cornelius, part of whose sarcophagus is now in the British Museum (6950). This, I take it, is the meaning of the allusions which Dr. Wissowa quotes (p. 298).

FRANK GRANGER.

Revue numismatique. Part 2, 1902.

Blanchet, 'Recherches sur les monnaies celtiques de l'Europe centrale.'—Tacchella. 'Numismatique de Philippiopolis.'—Mowat, 'Les essais monétaires de répétition et la division du travail.' Gives a useful descriptive list of Roman coins, Republican and Imperial, on which an obverse or reverse type is twice repeated on the same coin. We find bronze coins of Nero, Trajan, Antoninus Pius, &c., with the head of the Emperor on the reverse as well as on the obverse, and the reverse of a denarius of the Appuleius gens appears both as the 'obverse' and the 'reverse' of the coin. But though the type is repeated, the legend on obv. and rev. is not always the same; thus, we find such discrepancies as *Obv.* bust of Trajan cos v. *Rev.* Same bust cos vi. Nor are even the two heads always identical in detail. Mowat's explanation (which is hardly sufficiently developed) appears to be that these exceptional pieces were not intended for circulation, but were models or proof-impressions used at the Mint for the guidance and instruction of engravers who had to copy the heads and prepare the inscriptions.—Maurice. 'L'atelier monétaire de Carthage pendant la période Constantinienne.'—Héron de Villefosse. 'Le grand autel de Pergame sur un médaillon de bronze trouvé en France.' The paper read before the Academy of Inscriptions (see the *Comptes Rendus*, 1901, p. 823). The coin is a large medallion of Severus and Domna, of which a specimen was

published in *Brit. Mus. Cat., Mysia*, pl. xxx. 7. It shows an altar raised on steps, a colonnade and, in front, two bulls on pedestals. The identification with the Great Altar, though not without some difficulties, seems very probable.—Rouvier. 'Les rois Phéniciens de Sidon d'après leurs monnaies sous la dynastie des Achéménides' (5th and 4th centuries B.C.).—Reviews. Macdonald's 'Greek coins in the Hunterian Collection,' Vol. ii.

Numismatic Chronicle. Part 2, 1902.

Sir H. Howorth. 'A note on some coins generally attributed to Mazaios the satrap of Cilicia and Syria.' The coins are described in *Brit. Mus. Cat., Lycaonia, &c.*, under Tarsus, Nos. 65-75, and are assigned by Mr. Hill to the time of Mazaios. They are uninscribed (other coins of Mazaios have Aramaic inscriptions) and Howorth is of opinion that they were struck, not by Mazaios but by the Greek governors of Cilicia appointed by Alexander and his successors.—Sir John Evans. 'The burning of bonds under Hadrian.' A brief description of the well-

known sestertii of Hadrian with the inscription *Reliqua vetera HS. novies mill. abolita.* Reference is made to the sculptured Plutei in the Roman Forum with the representation of the burning (in the Forum Romanum) of the registers, apparently in the reign of Trajan. The burning of the bonds under Hadrian took place, according to Spartan, in the Forum Trajanum.—J. Maurice. 'Classification chronologique des émissions monétaires de l'atelier d'Alexandrie pendant la période Constantinienne'—*Miscellanea.* Th. Reinach corrects a statement in his paper (*Num. Chron.*, 1902) 'On some Pontic Eras.' The Era of Sebastopolis-Heracleopolis has its starting-point in Oct., 3 B.C., not, as previously stated, Oct., 2 B.C.—Haverfield describes three hoards of Roman coins, (i) Third brass, Valerian—Aurelian. Found near Birling Gap, Sussex, 1879. (ii) About 4,000 small brass, Gallienus—Constans. Found near Easton, Norfolk, 1851. (iii) 20 denarii, Tiberius—Faustina, jun., found in an urn in 1895, near Caistor, Norfolk.

WARWICK WROTH.

SUMMARIES OF PERIODICALS.

Revue de Philologie. Vol. 26, 3. 1902.

Kόρον πεδίον. B. Keil. On two Bithynian inscriptions referring to the battle of Corpedion. *La 'Nenia.'* H. de la Ville de Mirmont. An historical account of the *Nenia* in Roman classical and later literature. *Les πατρόβουλοι dans l'épigraphie grecque et la littérature talmudique,* I. Lévy. Maintains that *πατρόβουλοι* had no connexion with the Roman *patroni*. The office of *πατρόβουλος* arose spontaneously from circumstances common to the two halves of the empire. *Ad Rev. de Phil.* 26, pp. 224 sqq., F. Hiller von Gaertringen. Produces an inscr. of the third century A.D. on *πατρόβουλοι*, overlooked by Cumont. *Ἀπότολμος = audax non inaudax,* C. E. Ruelle. *Ubi ferrum nascitur,* F. Cumont. On the meaning of this phrase in a dedication to Jupiter Dolichenos. *La conjonction cum. Nouvelles remarques,* F. Gaffiot. A reply to Lebreton in the last no. Maintains that in classical Latin the temporal sense remains confined to the indic. *Un nouveau document relatif à la confédération des Cyclades,* J. Delamarre. On an inscr. discovered in 1860 at Heraclea, a small island between Naxos and Amorgos, and now at Amorgos. *L'influence macédonienne dans les Cyclades du III^e siècle avant J.C.*, J. Delamarre. Evidences of this influence as seen in various inscriptions.

Neue Jahrbücher für das Klassische Altertum, etc. Vol. 9, 3. 1902.

F. Leo. *Zur neuesten Bewegung in der griechischen Metrik.* The objections to the latest metrical theories (see the works of Masqueray and Gleditsch) are too little considered by the founders of theories. These objections must be removed by historical rather than by scientific methods. E. Lamert, *Die Entwicklung der römischen Taktik.* Deals with the disappearance of the division of the army by classes, and shows the development of the arrangement (1) by maniples, (2) by cohorts. R. Helm, *Lucian und die Philosophenschulen.* Deals with the relation of Lucian to the various philosophical schools, and his derision of their learning. E. Ziebarth, *Cyriacus von Ancona*

als Begründer der Inschriftenforschung. On the life and work of Ciriaco Pizzecolli of Ancona, who, after the early beginnings of the anonymous Einsidensis and the Sylloge Signoriana founded by Rienzi, began to collect the evidence over the whole field of antiquity. J. Ilberg, *Die Büchse der Pandora.*

Part 4. A. Bauer, *Neue Bücher zur griechischen Geschichte.* (1) On E. Meyer's *Geschichte des Altertums*, vol. 3. Does not agree with Ullrich's hypothesis of the Archidamian War as an independent work of Thucydides. (2) On K. Breysig's *Kulturgeschichte der Neuzeit.* M. Pokrowski, *Beiträge zur Charakteristik Ovids* (transl. from the Russian by E. Berneker). Seeks to show Ovid's slavish imitation of his Alexandrian models. Ovid's treatment of the Lucretius episode contrasted with Livy's. R. Helm, *Lucian und die Philosophenschulen* (continued). Lucian treats the Epicureans with good-will while he throws contempt on the Stoics.

Part 5. B. Delbrück, *Die Grundbegriffe der Kasus und Modi.* With reference to Morris' *Principles and Methods in Latin Syntax.* The spoken language has not the least consciousness of a ground-meaning of the cases. The investigation must proceed upwards and downwards from the Indo-Germanic types. The ground-notion of the subjunctive is will. A. Bauer, *Neue Bücher zur griechischen Geschichte* (concluded). Criticizes Pöhlmann's *Geschichte des antiken Kommunismus und Sozialismus I.* Guiraud's *La main d'œuvre dans l'ancienne Grèce*, Francotte's *L'industrie dans la Grèce ancienne*, Delbrück's *Geschichte der Kriegskunst I.*, and some essays from Lehmann's *Beiträge zur alten Geschichte.* R. Helm, *Lucian und die Philosophenschulen* (concluded). L. agrees most of all with the Cynics, just because they are in the least degree philosophers. L. Deubner, *Juturna und die Ausgrabungen auf dem römischen Forum.* O. Seeck, *Der Hildesheimer Silberfund.* It is quite probable that this silver treasure belonged to the booty in the fight with Varus, that previously one of the chief Roman officers was its possessor, and that it afterwards came into the hands of one of the leaders of the Cherusci.

Mnemosyne. Vol. 30, 3. 1902.

J. van Leeuwen, *Quis furor?* A reply to Roemer's *Studien zu Aristophanes*, who there attacks Van L. H. v. H., *Aristoph.* Eq. 504 sqq. S. A. Naber, *Observationes criticae ad Dionysii Halicarnassensis Antiquitates Romanas.* Critical notes. J. J. H., *Plut.* Solon. 10. K. G. P. Schwartz, *Plut. Lyc.* 27. J. Vürtheim, *De Amazonibus.* The native country of the Amazons is not to be sought in the East or in Scythia, but in Greece itself. K. Kuiper, *De Matre Magna Pergamenorum.* Originally it was the Idaean not the Pessinuntian Mother that came from Pergamus. About B.C. 102 came the worship of the Pessinuntian Mother (see *Plut. Mar.* 17). J. J. H., *Ad Plutarchum.* On Rom. 29 and Public. 22. H. van Herwerden, *Ad Alciphronis epistulas.* Critical notes. J. J. H., *Ad Plutarchum.* On Coriol. 32 and 38. J. C. Naber, *Observationes ad iure Romano. Ad formulam actionis arbitriae. Quid sit in rem actio.* J. v. L., *Ad Thucyd.* vii. 56. Suggests ἐκεὶ ἐν τῷ for ἐπειτα τῷ. J. J. Hartman, *Tacitea.* On various passages. M. L. Earle, *Ad Horat. Serm.* i. 1. 15 sqq. J. van Leeuwen, *Ad Aristophanis Plutum.* Critical notes.

Part 4. K. G. P. Schwartz, *Ad Lucianum.* Critical notes. H. J. Polak, *Paralipomena Lysiaca.* Critical notes. J. J. H., *Ad Plutarchum.* On Lyc. 27 and Nem. 1. J. J. Hartman, *Tacitea.* On various passages. J. v. L., *Horapoll.* i. 55. P. H. Damsté, *Ad Catulli Carmen 49.* Catullus was, and boasted that he was, one of the *Cantores Euphorionis.* The date of the poem is probably end of 63 or beginning of 62. J. van Leeuwen, *Ad Aristophanis Plutum.* Critical notes. S. A. Naber, *Ruhnkenii usor.*

Wochenschrift für Klassische Philologie. 1902.

25 June. *Prospopographia Attica*, ed. J. Kirchner. I. (H. Winther), favourable. J. Strzygowski, *Der Bilderkreis des griechischen Physiologus* (G. Thiele), very favourable.

2 July. W. Nestle, *Untersuchungen über die philosophischen Quellen des Euripides* (W. Gemoll). A worthy completion to the author's *Euripides, der Dichter der griech. Aufklärung.* G. Woerpel, *Quæstiōnēs de Lysiae oratione ὅτε τοῦ ἀδυτάρου* (E. Rosenberg). K. P. R. Neville, *The case construction after the comparative in Latin* (Fr. Fuguer), favourable. *Incerti scriptoris byzantini saec. X. liber de remilitari*, rec. R. Varsi (F. Hirsch) favourable.

9 July. W. Ridgeway, *The early age of Greece*, I. (O. Schrader). 'Though we may not agree with the conclusions, no one can lay down the book without

receiving great advantage.' E. Steinmeyer, *Beiträge zur Entstehungsgeschichte des Clm. 18140* (G. Pfeilschifter). 'A valuable piece of work.'

16 July. A. Baumgartner, *Geschichte der Weltlitteratur.* II. 3 und 4. Aufl. (A. L. Feder), very favourable. J. Sitzler, *Ein ästhetischer Kommentar zu Homers Odyssee* (G. Vogrizz), favourable on the whole. L. Bloch, *Alkestisstudien* (H. Morsch), favourable.

23 July. A. Mayr, *Die vorgeschichtlichen Denkmäler von Malta* and *Die altchristlichen Begräbnisstätten auf Malta* (F. v. Duhn), very favourable. A. Gercke, *Abriss der griechischen Lautlehre* (Bartholomaei), unfavourable. *Cicerō's Rede für Plancius*, herausg. von H. Nohl (W. Hirschfelder), favourable. C. Diehl, *Justinien et la civilisation Byzantine au V^{me} siècle* (C. Benjamin), very favourable.

6 Aug. E. Bethe, *Homer und die Heldenage* (Hoerenz), favourable. K. Bürger, *Studien zur Geschichte des griechischen Romans.* I. *Der Lukiosroman* (P. Schulze), favourable. T. R. Glover, *Life and letters in the fourth century* (J. Tolkein), favourable.

13 Aug. *Studies in honour of Basil L. Gildersleeve.* Contains 14 philological contributions. K. Praechter, *Hierokles der Stoiker* (A. Bonhöffer), very favourable. Der Hillesheimer Silberfund herausg. von E. Pernice und F. Winter (G. Körte). O. Bardenhewer, *Geschichte der altkirchlichen Litteratur.* I. (J. Dräseke). From the time of the Apostles to the end of the second century. 'Greatly furthers our knowledge.'

27 Aug. *Lysiae orationes*, rec. Th. Thalheim. 1. Ed. maior, 2. Ed. minor (E. Althaus), favourable. F. Skutsch, *Aus Vergili's Frühzeit* (G. Eskuche), unfavourable. H. d'Arbois de Jubainville, *Principaux auteurs de l'antiquité à consulter depuis les temps les plus anciens jusqu'au règne de Théodose.* I. = *Cours de littérature celtique XII* (A. Holder), favourable. *Symbolae in honorem L. Cwiklinski* (Z. Dembitzer).

3 Sep. *Poetarum graecorum fragmenta.* III. 1. *Poetarum philosophorum fragmenta*, ed. H. Diels (K. Praechter), very favourable. *Hermiae Alexandri in Platonis Phaedrum scholia*, ed. P. Couvreur (M. Wohlrab), favourable. *Tacitus, Lebenbeschreibung des Agricola*, herausg. von H. Smolka (R. Lange), favourable.

10 Sep. R. Wünsch, *Das Frühlingsfest der Insel Malta* (E. Samter), very favourable. *Papyri graeci musci britannici et musci berolinensis*, ed. a C. Kalbfleisch (W. Cröner). *Livi ab urbe condita libri*, ed. A. Zingerle, VII. 2 (lib. 42). Ed. maior. (W. Heraeus), favourable. *Claudii Hermeri Mulo-medicina Chironis*, ed. E. Oder (R. Fuchs), favourable.